

# SCIENCE

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## ADDRESS OF THE PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE<sup>1</sup>

At Melbourne I spoke of the new knowledge of the properties of living things which Mendelian analysis has brought us. I indicated how these discoveries are affecting our outlook on that old problem of natural history, the origin and nature of species, and the chief conclusion I drew was the negative one, that, though we must hold to our faith in the evolution of species, there is little evidence as to how it has come about, and no clear proof that the process is continuing in any considerable degree at the present time. The thought uppermost in our minds is that knowledge of the nature of life is altogether too slender to warrant speculation on these fundamental subjects. Did we presume to offer such speculations they would have no more value than those which alchemists might have made as to the nature of the elements. But though in regard to these theoretical aspects we must confess to such deep ignorance, enough has been learned of the general course of heredity within a single species to justify many practical conclusions which can not in the main be shaken. I propose now to develop some of these conclusions in regard to our own species, man.

In my former address I mentioned the condition of certain animals and plants which are what we call "polymorphic." Their populations consist of individuals of many types, though they breed freely together with perfect fertility. In cases of

<sup>1</sup> Second part of the address delivered at Sydney on August 20. The first part of the address, delivered at Melbourne on August 14, was printed in the last issue of SCIENCE.

this kind which have been sufficiently investigated it has been found that these distinctions—sometimes very great and affecting most diverse features of organization—are due to the presence or absence of elements, or factors, as we call them, which are treated in heredity as separate entities. These factors and their combinations produce the characteristics which we perceive. No individual can acquire a particular characteristic unless the requisite factors entered into the composition of that individual at fertilization, being received either from the father or from the mother or from both, and consequently no individual can pass on to his offspring positive characters which he does not himself possess. Rules of this kind have already been traced in operation in the human species; and though I admit that an assumption of some magnitude is involved when we extend the application of the same system to human characteristics in general, yet the assumption is one which I believe we are fully justified in making. With little hesitation we can now declare that the potentialities and aptitudes, physical as well as mental, sex, colors, powers of work or invention, liability to diseases, possible duration of life, and the other features by which the members of a mixed population differ from each other, are determined from the moment of fertilization; and by all that we know of heredity in the forms of life with which we can experiment we are compelled to believe that these qualities are in the main distributed on a factorial system. By changes in the outward conditions of life the expression of some of these powers and features may be excited or restrained. For the development of some an external opportunity is needed, and if that be withheld the character is never seen, any more than if the body be starved can the full height be attained; but such influences are super-

ficial and do not alter the genetic constitution.

The factors which the individual receives from his parents and no others are those which he can transmit to his offspring; and if a factor was received from one parent only, not more than half the offspring, on an average, will inherit it. What is it that has so long prevented mankind from discovering such simple facts? Primarily the circumstance that as man must have *two* parents it is not possible quite easily to detect the contributions of each. The individual body is a *double* structure, whereas the germ-cells are *single*. Two germ-cells unite to produce each individual body, and the ingredients they respectively contribute interact in ways that leave the ultimate product a medley in which it is difficult to identify the several ingredients. When, however, their effects are conspicuous the task is by no means impossible. In part also even physiologists have been blinded by the survival of ancient and obscurantist conceptions of the nature of man by which they were discouraged from the application of any rigorous analysis. Medical literature still abounds with traces of these archaisms, and, indeed, it is only quite recently that prominent horse-breeders have come to see that the dam matters as much as the sire. For them, though vast pecuniary considerations were involved, the old "homunculus" theory was good enough. We were amazed at the notions of genetic physiology which Professor Baldwin Spencer encountered in his wonderful researches among the natives of Central Australia; but in truth, if we reflect that these problems have engaged the attention of civilized man for ages, the fact that he, with all his powers of recording and deduction, failed to discover any part of the Mendelian system is almost as amazing. The popular notion that any parents can



have any kind of children within the racial limits is contrary to all experience, yet we have greatly entertained such ideas. As I have said elsewhere, the truth might have been found out at any period in the world's history if only pedigrees had been drawn the right way up. If, instead of exhibiting the successive pairs of progenitors who have contributed to the making of an ultimate individual, some one had had the idea of setting out the posterity of a single ancestor who possessed a marked feature such as the Hapsburg lip, and showing the transmission of this feature along some of the descending branches and the permanent loss of the feature in collaterals, the essential truth that heredity can be expressed in terms of presence and absence must have at once become apparent. For the descendant is not, as he appears in the conventional pedigree, a sort of pool into which each tributary ancestral stream has poured something, but rather a conglomerate of ingredient-characters taken from his progenitors in such a way that some ingredients are represented and others are omitted.

Let me not, however, give the impression that the unraveling of such descents is easy. Even with fairly full details, which in the case of man are very rarely to be had, many complications occur, often preventing us from obtaining more than a rough general indication of the system of descent. The nature of these complications we partly understand from our experience of animals and plants which are amenable to breeding under careful restrictions, and we know that they are mostly referable to various effects of interaction between factors by which the presence of some is masked.

Necessarily the clearest evidence of regularity in the inheritance of human characteristics has been obtained in regard to the descent of marked abnormalities of

structure and congenital diseases. Of the descent of ordinary distinctions such as are met with in the normal healthy population we know little for certain. Hurst's evidence, that two parents, both with light-colored eyes—in the strict sense, meaning that no pigment is present on the front of the iris—do not have dark-eyed children, still stands almost alone in this respect. With regard to the inheritance of other color-characteristics some advance has been made, but everything points to the inference that the genetics of color and many other features in man will prove exceptionally complex. There are, however, plenty of indications of system comparable with those which we trace in various animals and plants, and we are assured that to extend and clarify such evidence is only a matter of careful analysis. For the present, in asserting almost any general rules for human descent, we do right to make large reservations for possible exceptions. It is tantalizing to have to wait, but of the ultimate result there can be no doubt.

I spoke of complications. Two of these are worth illustrating here, for probably both of them play a great part in human genetics. It was discovered by Nilsson-Ehle, in the course of experiments with certain wheats, that several factors having the same power may co-exist in the same individual. These cumulative factors do not necessarily produce a cumulative effect, for any one of them may suffice to give the full result. Just as the pure-bred tall pea with its two factors for tallness is no taller than the cross-bred with a single factor, so these wheats with three pairs of factors for red color are no redder than the ordinary reds of the same family. Similar observations have been made by East and others. In some cases, as in the primulas studied by Gregory, the effect is cumulative. These

results have been used with plausibility by Davenport and the American workers to elucidate the curious case of the mulatto. If the descent of color in the cross between the negro and the white man followed the simplest rule, the offspring of two first-cross mulattoes would be, on an average, one black: two mulattoes: one white, but this is notoriously not so. Evidence of some segregation is fairly clear, and the deficiency of real whites may perhaps be accounted for on the hypothesis of cumulative factors, though by the nature of the case strict proof is not to be had. But at present I own to a preference for regarding such examples as instances of imperfect segregation. The series of germ-cells produced by the cross-bred consists of some with no black, some with full black, and others with intermediate quantities of black. No statistical tests of the condition of the gametes in such cases exist, and it is likely that by choosing suitable crosses all sorts of conditions may be found, ranging from the simplest case of total segregation, in which there are only two forms of gametes, up to those in which there are all intermediates in various proportions. This at least is what general experience of hybrid products leads me to anticipate. Segregation is somehow effected by the rhythms of cell-division, if such an expression may be permitted. In some cases the whole factor is so easily separated that it is swept out at once; in others it is so intermixed that gametes of all degrees of purity may result. That is admittedly a crude metaphor, but as yet we can not substitute a better. Be all this as it may, there are many signs that in human heredity phenomena of this kind are common, whether they indicate a multiplicity of cumulative factors or imperfections in segregation. Such phenomena, however, in no way detract from the essential truth that segregation occurs, and that the or-

ganism can not pass on a factor which it has not itself received.

In human heredity we have found some examples, and I believe that we shall find many more, in which the descent of factors is limited by sex. The classical instances are those of color-blindness and hæmophilia. Both these conditions occur with much greater frequency in males than in females. Of color-blindness at least we know that the *sons* of the color-blind man do not inherit it (unless the mother is a transmitter) and do not transmit it to their children of either sex. Some, probably all, of the daughters of the color-blind father inherit the character, and though not themselves color-blind, they transmit it to some (probably, on an average, half) of their offspring of both sexes. For since these normal-sighted women have only received the color-blindness from one side of their parentage, only half their offspring, on an average, can inherit it. The sons who inherit the color-blindness will be color-blind, and the inheriting daughters become themselves again transmitters. Males with normal color-vision, whatever their own parentage, do not have color-blind descendants, unless they marry transmitting women. There are points still doubtful in the interpretation, but the critical fact is clear, that the germ-cells of the color-blind man are of two kinds: (i) those which do not carry on the affection and are destined to take part in the formation of sons; and (ii) those which do carry on the color-blindness and are destined to form daughters. There is evidence that the ova also are similarly predestined to form one or other of the sexes, but to discuss the whole question of sex-determination is beyond my present scope. The descent of these sex-limited affections, nevertheless, calls for mention here, because it is an admirable illustration of factorial pre-



destination. It, moreover, exemplifies that *parental polarity* of the zygote to which I alluded in my first address, a phenomenon which we suspect to be at the bottom of various anomalies of heredity, and suggests that there may be truth in the popular notion that in some respects sons resemble their mothers and daughters their fathers.

As to the descent of hereditary diseases and malformations, however, we have abundant data for deciding that many are transmitted as dominants and a few as recessives. The most remarkable collection of these data is to be found in family histories of diseases of the eye. Neurology and dermatology have also contributed many very instructive pedigrees. In great measure the ophthalmological material was collected by Edward Nettleship, for whose death we so lately grieved. After retiring from practise as an oculist he devoted several years to this most laborious task. He was not content with hearsay evidence, but traveled incessantly, personally examining all accessible members of the families concerned, working in such a way that his pedigrees are models of orderly observation and recording. His zeal stimulated many younger men to take part in the work, and it will now go on, with the result that the systems of descent of all the common hereditary diseases of the eye will soon be known with approximate accuracy.

Give a little imagination to considering the chief deduction from this work. Technical details apart, and granting that we can not wholly interpret the numerical results, sometimes noticeably more and sometimes fewer descendants of these patients being affected than Mendelian formulæ would indicate, the expectation is that in the case of many diseases of the eye a large proportion of the children, grandchildren, and remoter descendants of the patients will be affected with the disease. Some-

times it is only defective sight that is transmitted; in other cases it is blindness, either from birth or coming on at some later age. The most striking example, perhaps, is that of a form of night-blindness still prevalent in a district near Montpellier, which has affected at least 130 persons, all descending from a single affected individual<sup>2</sup> who came into the country in the seventeenth century. The transmission is in every case through an affected parent, and no normal has been known to pass on the condition. Such an example well serves to illustrate the fixity of the rules of descent. Similar instances might be recited relating to a great variety of other conditions, some trivial, others grave.

At various times it has been declared that men are born equal, and that the inequality is brought about by unequal opportunities. Acquaintance with the pedigrees of disease soon shows the fatuity of such fancies. The same conclusion, we may be sure, would result from the true representation of the descent of any human faculty. Never since Galton's publications can the matter have been in any doubt. At the time he began to study family histories even the broad significance of heredity was frequently denied, and resemblances to parents or ancestors were looked on as interesting curiosities. Inveighing against hereditary political institutions, Tom Paine remarks that the idea is as absurd as that of an "hereditary wise man," or an "hereditary mathematician," and to this day I suppose many people are not aware that

<sup>2</sup> The first human descent proved to follow Mendelian rules was that of a serious malformation of the hand studied by Farabee in America. Drinkwater subsequently worked out pedigrees for the same malformation in England. After many attempts, he now tells me that he has succeeded in proving that the American family and one of his own had an abnormal ancestor in common, five generations ago.

he is saying anything more than commonly foolish. We, on the contrary, would feel it something of a puzzle if two parents, both mathematically gifted, had any children *not* mathematicians. Galton first demonstrated the overwhelming importance of these considerations, and had he not been misled, partly by the theory of pangenesis, but more by his mathematical instincts and training, which prompted him to apply statistical treatment rather than qualitative analysis, he might, not improbably, have discovered the essential facts of Mendelism.

It happens rarely that science has anything to offer to the common stock of ideas at once so comprehensive and so simple that the courses of our thoughts are changed. Contributions to the material progress of mankind are comparatively frequent. They result at once in application. Transit is quickened; communication is made easier; the food-supply is increased and population multiplied. By direct application to the breeding of animals and plants such results must even flow from Mendel's work. But I imagine the greatest practical change likely to ensue from modern genetic discovery will be a quickening of interest in the true nature of man and in the biology of races. I have spoken cautiously as to the evidence for the operation of any simple Mendelian system in the descent of human faculty; yet the certainty that systems which differ from the simpler schemes only in degree of complexity are at work in the distribution of characters among the human population can not fail to influence our conceptions of life and of ethics, leading perhaps ultimately to modification of social usage. That change can not but be in the main one of simplification. The eighteenth century made great pretence of a return to nature, but it did not occur to those philos-

ophers first to inquire what nature is; and perhaps not even the patristic writings contain fantasies much further from physiological truth than those which the rationalists of the "Encyclopædia" adopted as the basis of their social schemes. For men are so far from being born equal or similar that to the naturalist they stand as the very type of a polymorphic species. Even most of our local races consist of many distinct strains and individual types. From the population of any ordinary English town as many distinct human breeds could in a few generations be isolated as there are now breeds of dogs, and indeed such a population in its present state is much what the dogs of Europe would be in ten years' time but for the interference of the fanciers. Even as at present constituted, owing to the isolating effects of instinct, fashion, occupation and social class, many incipient strains already exist.

In one respect civilized man differs from all other species of animal or plant in that, having prodigious and ever-increasing power over nature, he invokes these powers for the preservation and maintenance of many of the inferior and all the defective members of his species. The inferior freely multiply, and the defective, if their defects be not so grave as to lead to their detention in prisons or asylums, multiply also without restraint. Heredity being strict in its action, the consequences are in civilized countries much what they would be in the kennels of the dog-breeder who continued to preserve all his puppies, good and bad: the proportion of defectives increases. The increase is so considerable that outside every great city there is a smaller town inhabited by defectives and those who wait on them. Round London we have a ring of such towns with some 30,000 inhabitants, of whom about 28,000 are defective,



largely, though of course by no means entirely, bred from previous generations of defectives. Now, it is not for us to consider practical measures. As men of science we observe natural events and deduce conclusions from them. I may perhaps be allowed to say that the remedies proposed in America, in so far as they aim at the eugenic regulation of marriage on a comprehensive scale, strike me as devised without regard to the needs either of individuals or of a modern state. Undoubtedly if they decide to breed their population of one uniform puritan gray, they can do it in a few generations; but I doubt if timid respectability will make a nation happy, and I am sure that qualities of a different sort are needed if it is to compete with more vigorous and more varied communities. Every one must have a preliminary sympathy with the aims of eugenists both abroad and at home. Their efforts at the least are doing something to discover and spread truth as to the physiological structure of society. The spirit of such organizations, however, almost of necessity suffers from a bias towards the accepted and the ordinary, and if they had power it would go hard with many ingredients of society that could be ill-spared. I notice an ominous passage in which even Galton, the founder of eugenics, feeling perhaps some twinge of his Quaker ancestry, remarks that "as the Bohemianism in the nature of our race is destined to perish, the sooner it goes, the happier for mankind." It is not the eugenists who will give us what Plato has called divine releases from the common ways. If some fancier with the catholicity of Shakespeare would take us in hand, well and good; but I would not trust even Shakespeares meeting as a committee. Let us remember that Beethoven's father was a habitual drunkard and that his mother died of consumption. From the genealogy

of the patriarchs also we learn—what may very well be the truth—that the fathers of such as dwell in tents, and of all such as handle the harp or organ, and the instructor of every artificer in brass and iron—the founders, that is to say, of the arts and the sciences—came in direct descent from Cain, and not in the posterity of the irreproachable Seth, who is to us, as he probably was also in the narrow circle of his own contemporaries, what naturalists call a *nomen nudum*.

Genetic research will make it possible for a nation to elect by what sort of beings it will be represented not very many generations hence, much as a farmer can decide whether his byres shall be full of short-horns or Herefords. It will be very surprising indeed if some nation does not make trial of this new power. They may make awful mistakes, but I think they will try.

Whether we like it or not, extraordinary and far-reaching changes in public opinion are coming to pass. Man is just beginning to know himself for what he is—a rather long-lived animal, with great powers of enjoyment if he does not deliberately forego them. Hitherto superstition and mythical ideas of sin have predominantly controlled these powers. Mysticism will not die out: for those strange fancies knowledge is no cure; but their forms may change, and mysticism as a force for the suppression of joy is happily losing its hold on the modern world. As in the decay of earlier religions Ushabti dolls were substituted for human victims, so telepathy, necromancy and other harmless toys take the place of eschatology and the inculcation of a ferocious moral code. Among the civilized races of Europe we are witnessing an emancipation from traditional control in thought, in art, and in conduct which is likely to have prolonged and wonderful influences. Returning to freer or, if you will, simpler con-

ceptions of life and death, the coming generations are determined to get more out of this world than their forefathers did. Is it then to be supposed that when science puts into their hand means for the alleviation of suffering immeasurable, and for making this world a happier place, that they will demur to using those powers? The intenser struggle between communities is only now beginning, and with the approaching exhaustion of that capital of energy stored in the earth before man began it must soon become still more fierce. In England some of our great-grandchildren will see the end of the easily accessible coal, and, failing some miraculous discovery of available energy, a wholesale reduction in population. There are races who have shown themselves able at a word to throw off all tradition and take into their service every power that science has yet offered them. Can we expect that they, when they see how to rid themselves of the ever-increasing weight of a defective population, will hesitate? The time can not be far distant when both individuals and communities will begin to think in terms of biological fact, and it behooves those who lead scientific thought carefully to consider whither action should lead. At present I ask you merely to observe the facts. The powers of science to preserve the defective are now enormous. Every year these powers increase. This course of action must reach a limit. To the deliberate intervention of civilization for the preservation of inferior strains there must sooner or later come an end, and before long nations will realize the responsibility they have assumed in multiplying these "cankers of a calm world and a long peace."

The definitely feeble-minded we may with propriety restrain, as we are beginning to do even in England, and we may safely prevent unions in which both parties

are defective, for the evidence shows that as a rule such marriages, though often prolific, commonly produce no normal children at all. The union of such social vermin we should no more permit than we would allow parasites to breed on our own bodies. Further than that in restraint of marriage we ought not to go, at least not yet. Something too may be done by a reform of medical ethics. Medical students are taught that it is their duty to prolong life at whatever cost in suffering. This may have been right when diagnosis was uncertain and interference usually of small effect; but deliberately to interfere now for the preservation of an infant so gravely diseased that it can never be happy or come to any good is very like wanton cruelty. In private few men defend such interference. Most who have seen these cases lingering on agree that the system is deplorable, but ask where can any line be drawn. The biologist would reply that in all ages such decisions have been made by civilized communities with fair success both in regard to crime and in the closely analogous case of lunacy. The real reason why these things are done is because the world collectively cherishes occult views of the nature of life, because the facts are realized by few, and because between the legal mind—to which society has become accustomed to defer—and the seeing eye, there is such physiological antithesis that hardly can they be combined in the same body. So soon as scientific knowledge becomes common property, views more reasonable and, I may add, more humane, are likely to prevail.

To all these great biological problems that modern society must sooner or later face there are many aspects besides the obvious ones. Infant mortality we are asked to lament without the slightest thought of what the world would be like if



the majority of these infants were to survive. The decline in the birth-rate in countries already over-populated is often deplored, and we are told that a nation in which population is not rapidly increasing must be in a decline. The slightest acquaintance with biology, or even school-boy natural history, shows that this inference may be entirely wrong, and that before such a question can be decided in one way or the other, hosts of considerations must be taken into account. In normal stable conditions population is stationary. The laity never appreciates, what is so clear to a biologist, that the last century and a quarter, corresponding with the great rise in population, has been an altogether exceptional period. To our species this period has been what its early years in Australia were to the rabbit. The exploitation of energy-capital of the earth in coal, development of the new countries, and the consequent pouring of food into Europe, the application of antiseptics, these are the things that have enabled the human population to increase. I do not doubt that if population were more evenly spread over the earth it might increase very much more; but the essential fact is that under any stable conditions a limit must be reached. A pair of wrens will bring off a dozen young every year, but each year you will find the same number of pairs in your garden. In England the limit beyond which under present conditions of distribution increase of population is a source of suffering rather than of happiness has been reached already. Younger communities living in territories largely vacant are very probably right in desiring and encouraging more population. Increase may, for some temporary reason, be essential to their prosperity. But those who live, as I do, among thousands of creatures in a state of semi-starvation will realize

that too few is better than too many, and will acknowledge the wisdom of Ecclesiasticus, who said, "Desire not a multitude of unprofitable children."

But at least it is often urged that the decline in the birth-rate of the intelligent and successful sections of the population—I am speaking of the older communities—is to be regretted. Even this can not be granted without qualification. As the biologist knows, differentiation is indispensable to progress. If population were homogeneous civilization would stop. In every army the officers must be comparatively few. Consequently, if the upper strata of the community produce more children than will recruit their numbers some must fall into the lower strata and increase the pressure there. Statisticians tell us that an average of four children under present conditions is sufficient to keep the number constant, and as the expectation of life is steadily improving we may perhaps contemplate some diminution of that number without alarm.

In the study of history biological treatment is only beginning to be applied. For us the causes of the success and failure of races are physiological events, and the progress of man has depended upon a chain of these events, like those which have resulted in the "improvement" of the domesticated animals and plants. It is obvious, for example, that had the cereals never been domesticated cities could scarcely have existed. But we may go further, and say that in temperate countries of the Old World (having neither rice nor maize) populations concentrated in large cities have been made possible by the appearance of a "thrashable" wheat. The ears of the wild wheats break easily to pieces, and the grain remains in the thick husk. Such wheat can be used for food, but not readily. Ages before written his-

tory began, in some unknown place, plants, or more likely a plant, of wheat lost the dominant factor to which this brittleness is due, and the recessive, thrashable wheat resulted. Some man noticed this wonderful novelty, and it has been disseminated over the earth. The original variation may well have occurred once only, in a single germ-cell.

So must it have been with man. Translated into terms of factors, how has that progress in control of nature which we call civilization been achieved? By the sporadic appearance of variations, mostly, perhaps all, consisting in a loss of elements, which inhibit the free working of the mind. The members of civilized communities, when they think about such things at all, imagine the process a gradual one, and that they themselves are active agents in it. Few, however, contribute anything but their labor; and except in so far as they have freedom to adopt and imitate, their physiological composition is that of an earlier order of beings. Annul the work of a few hundreds—I might almost say scores—of men, and on what plane of civilization should we be? We should not have advanced beyond the medieval stage without printing, chemistry, steam, electricity, or surgery worthy the name. These things are the contributions of a few excessively rare minds. Galton reckoned those to whom the term “illustrious” might be applied as one in a million, but in that number he is, of course, reckoning men famous in ways which add nothing to universal progress. To improve by subordinate invention, to discover details missed, even to apply knowledge never before applied, all these things need genius in some degree, and are far beyond the powers of the average man of our race; but the true pioneer, the man whose penetration creates a new world, as did that of

Newton and of Pasteur, is inconceivably rare. But for a few thousands of such men, we should perhaps be in the Paleolithic era, knowing neither metals, writing, arithmetic, weaving, nor pottery.

In the history of art the same is true, but with this remarkable difference, that not only are gifts of artistic creation very rare, but even the faculty of artistic enjoyment, not to speak of higher powers of appreciation, is not attained without variation from the common type. I am speaking, of course, of the non-Semitic races of modern Europe, among whom the power, whether of making or enjoying works of art, is confined to an insignificant number of individuals. Appreciation can in some degree be simulated, but in our population there is no widespread physiological appetite for such things. When detached from the centers where they are made by others most of us pass our time in great contentment, making nothing that is beautiful, and quite unconscious of any deprivation. Musical taste is the most notable exception, for in certain races—for example, the Welsh and some of the Germans—it is almost universal. Otherwise artistic faculty is still sporadic in its occurrence. The case of music well illustrates the application of genetic analysis to human faculty. No one disputes that musical ability is congenital. In its fuller manifestation it demands sense of rhythm, ear, and special nervous and muscular powers. Each of these is separable and doubtless genetically distinct. Each is the consequence of a special departure from the common type. Teaching and external influences are powerless to evoke these faculties, though their development may be assisted. The only conceivable way in which the people of England, for example, could become a musical nation would be by the gradual rise in the proportional numbers of a musical strain or



strains until the present type became so rare as to be negligible. It by no means follows that in any other respect the resulting population would be distinguishable from the present one. Difficulties of this kind beset the efforts of anthropologists to trace racial origins. It must continually be remembered that most characters are independently transmitted and capable of much recombination. In the light of Mendelian knowledge the discussion whether a race is pure or mixed loses almost all significance. A race is pure if it breeds pure, and not otherwise. Historically we may know that a race like our own was, as a matter of fact, of mixed origin. But a character may have been introduced by a single individual, though subsequently it becomes common to the race. This is merely a variant on the familiar paradox that in the course of time, if registration is accurate, we shall all have the same surname. In the case of music, for instance, the gift, originally, perhaps, from a Welsh source, might permeate the nation, and the question would then arise whether the nation, so changed, was the English nation or not.

Such a problem is raised in a striking form by the population of modern Greece, and especially of Athens. The racial characteristics of the Athenian of the fifth century B.C. are vividly described by Galton in "Hereditary Genius." The fact that in that period a population, numbering many thousands, should have existed, capable of following the great plays at a first hearing, reveling in subtleties of speech, and thrilling with passionate delight in beautiful things, is physiologically a most singular phenomenon. On the basis of the number of illustrious men produced by that age Galton estimated the average intelligence as at least two of his degrees above our own, differing from us as much

as we do from the negro. A few generations later the display was over. The origin of that constellation of human genius which then blazed out is as yet beyond all biological analysis, but I think we are not altogether without suspicion of the sequence of the biological events. If I visit a poultry-breeder who has a fine stock of thoroughbred game fowls breeding true, and ten years later—that is to say ten fowl-generations later—I go again and find scarcely a recognizable game-fowl on the place, I know exactly what has happened. One or two birds of some other or of no breed must have strayed in and their progeny been left undestroyed. Now in Athens we have many indications that up to the beginning of the fifth century, so long as the phratries and gentes were maintained in their integrity, there was rather close endogamy, a condition giving the best chance of producing a homogeneous population. There was no lack of material from which intelligence and artistic power might be derived. Sporadically these qualities existed throughout the ancient Greek world from the dawn of history, and, for example, the vase-painters, the makers of the Tanagra figurines, and the gem-cutters were presumably pursuing family crafts, much as are the actor-families<sup>3</sup> of England or the professorial families of Germany at the present day. How the intellectual strains should have acquired predominance we can not tell, but in an inbreeding community homogeneity at least is not surprising. At the end of the sixth century came the "reforms" of Cleisthenes (507 B.C.), which sanctioned foreign marriages and admitted to citizenship a number not only of resident aliens, but also of manumitted slaves. As Aristotle says, Cleisthenes legislated with the deliberate pur-

<sup>3</sup> For tables of these families, see the Supplement to "Who's Who in the Theater."

pose of breaking up the phratries and gentes, in order that the various sections of the population might be mixed up as much as possible, and the old tribal associations abolished. The "reform" was probably a recognition and extension of a process already begun; but is it too much to suppose that we have here the effective beginning of a series of genetic changes which in a few generations so greatly altered the character of the people? Under Pericles the old law was restored (451 B.C.), but losses in the great wars led to further laxity in practise, and though at the end of the fifth century the strict rule was re-enacted that a citizen must be of citizen-birth on both sides, the population by that time may well have become largely mongrelized.

Let me not be construed as arguing that mixture of races is an evil: far from it. A population like our own, indeed, owes much of its strength to the extreme diversity of its components, for they contribute a corresponding abundance of aptitudes. Everything turns on the nature of the ingredients brought in, and I am concerned solely with the observation that these genetic disturbances lead ultimately to great and usually unforeseen changes in the nature of the population.

Some experiments of this kind are going on at the present time, in the United States, for example, on a very large scale. Our grandchildren may live to see the characteristics of the American population entirely altered by the vast invasion of Italian and other South European elements. We may expect that the Eastern States, and especially New England, whose people still exhibit the fine Puritan qualities with their appropriate limitations, absorbing little of the alien elements, will before long be in feelings and aptitudes very notably differentiated from the rest. In Japan,

also, with the abolition of the feudal system and the rise of commercialism, a change in population has begun which may be worthy of the attention of naturalists in that country. Till the revolution the Samurai almost always married within their own class, with the result, as I am informed, that the caste had fairly recognizable features. The changes of 1868 and the consequent impoverishment of the Samurai have brought about a beginning of disintegration which may not improbably have perceptible effects.

How many genetic vicissitudes has our own peerage undergone! Into the hard-fighting stock of mediæval and Plantagenet times have successively been crossed the cunning shrewdness of Tudor statesmen and courtiers, the numerous contributions of Charles II. and his concubines, reinforcing peculiar and persistent attributes which popular imagination especially regards as the characteristic of peers, ultimately the heroes of finance and industrialism. Definitely intellectual elements have been sporadically added, with rare exceptions, however, from the ranks of lawyers and politicians. To this aristocracy art, learning and science have contributed sparse ingredients, but these mostly chosen for celibacy or childlessness. A remarkable body of men, nevertheless; with an average "horse-power," as Samuel Butler would have said, far exceeding that of any random sample of the middle-class. If only man could be reproduced by budding what a simplification it would be! In vegetative reproduction heredity is usually complete. The Washington plum can be divided to produce as many identical individuals as are required. If, say, Washington, the statesman, or preferably King Solomon, could similarly have been propagated, all the nations of the earth could have been supplied with ideal rulers.



Historians commonly ascribe such changes as occurred in Athens, and will almost certainly come to pass in the United States, to conditions of life and especially to political institutions. These agencies, however, do little unless they are such as to change the breed. External changes may indeed give an opportunity to special strains, which then acquire ascendancy. The industrial developments which began at the end of the eighteenth century, for instance, gave a chance to strains till then submerged, and their success involved the decay of most of the old aristocratic families. But the demagogue who would argue from the rise of the one and the fall of the other that the original relative positions were not justifiable altogether mistakes the facts.

Conditions give opportunities, but cause no variations. For example, in Athens, to which I just referred, the universality of cultivated discernment could never have come to pass but for the institution of slavery which provided the opportunity, but slavery was in no sense a cause of that development, for many other populations have lived on slaves and remained altogether inconspicuous.

The long-standing controversy as to the relative importance of nature and nurture, to use Galton's "convenient jingle of words," is drawing to an end, and of the overwhelmingly greater significance of nature there is no longer any possibility of doubt. It may be well briefly to recapitulate the arguments on which naturalists rely in coming to this decision as regards both races and individuals. First, as regards human individuals, there is the common experience that children of the same parents reared under conditions sensibly identical may develop quite differently, exhibiting in character and aptitudes a segregation just as great as in their colors or hair-forms. Conversely, all the more marked

aptitudes have at various times appeared and not rarely reached perfection in circumstances the least favorable for their development. Next, appeal can be made to the universal experience of the breeder, whether of animals or plants, that strain is absolutely essential, that though bad conditions may easily enough spoil a good strain, yet that under the best conditions a bad strain will never give a fine result. It is faith, not evidence, which encourages educationists and economists to hope so greatly in the ameliorating effects of the conditions of life. Let us consider what they can do and what they can not. By reference to some sentences in a charming though pathetic book, "What Is, and What Might Be," by Mr. Edmond Holmes, which will be well known in the Educational Section, I may make the point of view of us naturalists clear. I take Mr. Holmes's pronouncement partly because he is an enthusiastic believer in the efficacy of nurture as opposed to nature, and also because he illustrates his views by frequent appeals to biological analogies which help us to a common ground. Wheat badly cultivated will give a bad yield, though, as Mr. Holmes truly says, wheat of the same strain in similar soil well cultivated may give a good harvest. But, having witnessed the success of a great natural teacher in helping unpromising peasant children to develop their natural powers, he gives us another botanical parallel. Assuming that the wild bullace is the origin of domesticated plums, he tells us that by cultivation the bullace can no doubt be improved so far as to become a better bullace, but by no means can the bullace be made to bear plums. All this is sound biology; but, translating these facts into the human analogy, he declares that the work of the successful teacher shows that with man the facts are otherwise, and that the *average* rustic child, whose normal

ideal is "bullacehood," can become the rare exception, developing to a stage corresponding with that of the plum. But the naturalist knows exactly where the parallel is at fault. For the wheat and the bullace are both breeding approximately true, whereas the human crop, like jute and various cottons, is in a state of polymorphic mixture. The population of many English villages may be compared with the crop which would result from sowing a bushel of kernels gathered mostly from the hedges, with an occasional few from an orchard. If any one asks how it happens that there are any plum-kernels in the sample at all, he may find the answer perhaps in spontaneous variation, but more probably in the appearance of a long-hidden recessive. For the want of that genetic variation, consisting probably, as I have argued, in loss of inhibiting factors, by which the plum arose from the wild form, neither food, nor education, nor hygiene can in any way atone. Many wild plants are half-starved through competition, and, transferred to garden soil, they grow much bigger; so good conditions might certainly enable the bullace population to develop beyond the stunted physical and mental stature they commonly attain, but plums they can never be. Modern statesmanship aims rightly at helping those who have got sown as wildings to come into their proper class; but let not any one suppose such a policy democratic in its ultimate effects, for no course of action can be more effective in strengthening the upper classes whilst weakening the lower.

In all practical schemes for social reform the congenital diversity, the essential polymorphism of all civilized communities must be recognized as a fundamental fact, and reformers should rather direct their efforts to facilitating and rectifying class-distinctions than to any futile attempt to abolish

them. The teaching of biology is perfectly clear. We are what we are by virtue of our differentiation. The value of civilization has in all ages been doubted. Since, however, the first variations were not strangled in their birth, we are launched on that course of variability of which civilization is the consequence. We can not go back to homogeneity again, and differentiated we are likely to continue. For a period measures designed to create a spurious homogeneity may be applied. Such attempts will, I anticipate, be made when the present unstable social state reaches a climax of instability, which may not be long hence. Their effects can be but evanescent. The instability is due not to inequality, which is inherent and congenital, but rather to the fact that in periods of rapid change like the present, convection-currents are set up such that the elements of the strata get intermixed and the apparent stratification corresponds only roughly with the genetic. In a few generations under uniform conditions these elements settle in their true levels once more.

In such equilibrium is content most surely to be expected. To the naturalist the broad lines of solution of the problems of social discontent are evident. They lie neither in vain dreams of a mystical and disintegrating equality, nor in the promotion of that malignant individualism which in older civilization has threatened mortification of the humbler organs, but rather in a physiological coordination of the constituent parts of the social organism. The rewards of commerce are grossly out of proportion to those attainable by intellect or industry. Even regarded as compensation for a dull life, they far exceed the value of the services rendered to the community. Such disparity as an incident of the abnormally rapid growth of popula-



tion is quite indefensible as a permanent social condition. Nevertheless, capital, distinguished as a provision for offspring, is a eugenic institution; and unless human instinct undergoes some profound and improbable variation, abolition of capital means the abolition of effort; but as in the body the power of independent growth of the parts is limited and subordinated to the whole, similarly in the community we may limit the powers of capital, preserving so much inequality of privilege as corresponds with physiological fact.

At every turn the student of political science is confronted with problems that demand biological knowledge for their solution. Most obviously is this true in regard to education, the criminal law, and all those numerous branches of policy and administration which are directly concerned with the physiological capacities of mankind. Assumptions as to what can be done and what can not be done to modify individuals and races have continually to be made, and the basis of fact on which such decisions are founded can be drawn only from biological study.

A knowledge of the facts of nature is not yet deemed an essential part of the mental equipment of politicians; but as the priest, who began in other ages as medicine-man, has been obliged to abandon the medical parts of his practise, so will the future behold the schoolmaster, the magistrate, the lawyer, and ultimately the statesman, compelled to share with the naturalist those functions which are concerned with the physiology of race.

WILLIAM BATESON

#### THE STATUS OF HYPOTHESES OF POLAR WANDERINGS

For the past century, hypotheses which postulate a wandering of the earth's axis of rotation within its body have been advocated by various geologists and biologists as an explana-

tion of past climatic and biotic changes. Astronomers, on the contrary, have in general been opposed to hypotheses of polar migration; for in their opinion, not only is there no astronomic evidence pointing toward such instability of axis, but extensive and progressive wanderings are regarded as mechanically impossible. Geologists and biologists may array facts which suggest such hypotheses, but the testing of their possibility is really a problem of mathematics, as much as are the movements of precession, and orbital perturbations. Notwithstanding this, a number of hypotheses concerning polar migration have been ingeniously elaborated and widely promulgated without their authors submitting them to these final tests, or in most cases even perceiving that an accordance with the known laws of mechanics was necessary. Others, of more logical mind, recognizing the need of mathematical justification, have thought to find a qualified support in the work of Kelvin and G. H. Darwin. The chief point of this paper lies in showing that the work of Darwin, instead of permitting hypotheses of polar wanderings, offers the most convincing proof which is available that migrations of the axis of the earth sufficiently extensive to be of geological importance have not occurred. Darwin, in his conclusion, granted the possibility that the pole may have worked its way in a devious course some  $10^\circ$  or  $15^\circ$  away from the geographic position which it held *at the consolidation of the earth*, and he states that it may as a maximum have been deflected from  $1^\circ$  to  $3^\circ$  in *any one geological period*. This extreme limit to migration was purposely based upon those assumptions which might be geologically possible and which would permit the greatest changes in the axis of rotation. A reexamination of those assumptions in the light of forty added years of geologic progress suggests that the actual changes have been much less and are more likely to be limited to a fraction of the maximum limits set by Darwin. His paper seems to have checked further speculation upon this subject in England, but, apparently unaware of its strictures, a number of continental geologists and biologists have car-

ried forward these ideas of polar wandering to the present day. The hypotheses have grown, each creator selecting facts and building up from his particular assortment a fanciful hypothesis of polar migration unrestrained even by the devious paths worked out by others.

If these varied and contradictory hypotheses were kept merely as exhibits of those strange creations of the mind which are stored in the museum of pseudo-science, there would be little present need for a discussion of the subject; but such is not the case. Able workers in the fields of natural science, a number of them deservedly of the first rank, overlooking the fatal mathematical objections, impressed by the apparent authority of the originators of some of the hypotheses, and assuming that these authors had made a thorough investigation, have, while treating the subject cautiously, still given it serious attention. This is especially true of that very elaborated scheme of a pendulating earth put forth by Reibisch and voluminously supported by Simroth. It has been brought to the attention of the American scientific public in a favorable review by R. E. Richardson in *SCIENCE*,<sup>1</sup> and more lately by Grabau, who discusses this and other hypotheses of polar migrations on pages 891-899 of his recent work on "The Principles of Stratigraphy." In this work in fact the only hypotheses of climatic change through geologic time which receive detailed treatment are those of polar migrations, while certain important hypotheses, such as those of possible changes in the deep oceanic circulation, or changes in solar radiation, receive no mention. Although Grabau states that the pendulation theory is still too new and too little tested to receive more than respectful attention, he nevertheless regards it as a working hypothesis which is likely to be of much value, and from the space he devotes to it clearly considers it of much importance. The writer's high opinion of Grabau's "Principles of Stratigraphy" has been expressed recently in *SCIENCE* and it is because of his estimation of the importance of that work that this article

<sup>1</sup> Vol. XXVIII., pp. 375-379, 1908.

is written. The wide degree to which the "Principles of Stratigraphy" will be studied in America during the next decade will spread equally widely these ideas of polar migrations. There is need in consequence that the lines of counter arguments should be definitely set forth.

What then, briefly, are these hypotheses of polar wanderings and on what kind of evidence do they rest?

Some seventy years ago the proofs were developed of recent continental glaciation in middle latitudes. This was in striking contrast with the floral evidence of the mid-Tertiary warm temperate climate which prevailed in Greenland and Spitzbergen. The recognition of these great climatic changes in late geologic times gave rise to the suggestion that a migration of the poles seemed the simplest means of accounting for them. Sir John Lubbock in 1848 communicated a paper to the Geological Society of London upon this subject and Sir Henry De la Beche discussed it in his presidential address in 1849. Mr. John Evans in his presidential address to the same Geological Society in 1876 recurred to it. Evans, after describing a system of geological upheaval and subsidence, evidently designed to produce a maximum effect in shifting the polar axis, asks:

Would not such a modification of form bring the axis of figure about 15° or 20° south of the present, and on the meridian of Greenwich—that is to say, midway between Greenland and Spitzbergen, and would not, eventually, the axis of rotation correspond in position with the axis of figure?

It was in answer to these questions that George H. Darwin wrote his conclusive paper.

We may pass next to the far more extravagant demands of the hypotheses framed in later years and in disregard of Darwin's work. The exact references are all given by Grabau.

In 1901 Reibisch proposed a theory of polar pendulation, *i. e.*, a back and forth migration of the poles along a certain well-defined path. An axis of oscillation he supposed to pass through Equador and Sumatra. These points have consequently never changed in latitude. The axis of rotation is supposed to



remain always at right angles to this oscillation axis but to shift within the earth north and south along the meridian of  $10^\circ$  east of Greenwich. Thus the north pole is supposed in the Pleistocene to have lain north of Scandinavia and to be now advancing in the direction of Bering Sea. In the Jurassic and Cretaceous the continent of Europe is supposed to have been in tropic latitudes. An examination of the work of Reibisch shows no mention of the astronomic side of the problem nor any reference to the work of G. H. Darwin. His argument rests chiefly on various facts in the distribution of animals and plants and also upon the submergence and emergence of certain regions.

Kreichgauer in 1902 produced a map of the polar wanderings through geologic time as worked out by him, in which he shows the poles actually changing place, the north pole migrating from the Antarctic in the Pre-Cambrian northerly through the Pacific Ocean, through Alaska and the Arctic Archipelago to Greenland, and thence to its present position.

Jacobitti on the other hand prefers a different path, his north pole lying in the South Atlantic in Cambrian times, thence moving easterly across South Africa, India, Australia, the Pacific Ocean, Canada and Greenland to its present location.

Dr. Heinrich Simroth, professor in the University of Leipzig, elaborated the hypothesis of Reibisch, publishing in 1907 a book of 564 pages on "Die Pendulations Theorie."

These hypotheses rest chiefly upon facts and interpretations regarding the distribution of plants and animals. Support for them is also sought in the nature of crust movements and in the geologic evidences of past climatic changes. Much of the evidence is vague in delimitation and in significance, some of it is not clearly applicable, some of it could be offset by opposing evidence, and all of it can be given other interpretations which find a better geologic basis and do not contravene the laws of mechanics.

The writer has examined in some detail the hypothesis of Reibisch and Simroth, since this is the one which has been most commended

to geologists. Most of the following criticisms are directed toward their work, but in a general way they apply to the other hypotheses also. The kind of evidence upon which Reibisch founds a hypothesis involving a new earth motion of which he is the discoverer is seen in the following statements. He locates his oscillation poles in Equador and Sumatra because of botanical writings which claim that the Tertiary floras of those regions were not modified by Pleistocene climatic changes. Archaic and related types of animals inhabit these two antipodal regions, preserved from extinction because of the constancy of climate surrounding these oscillation poles. The oscillation circle is at  $90^\circ$  to these poles, running north and south through Europe and Africa. In the vicinity of this circle, climatic changes, owing to the poles moving back and forth on this meridian, are stated to have repeatedly driven out the faunas and floras and made this region that which has promoted the greatest evolutionary progress. Any other possible mode of accounting for the evidence these lands lie near his north-south belts of greatest oscillatory climatic change and offset the arguments drawn from Equador and Sumatra. The early Tertiary fauna preserved in Madagascar needs especially some explanation since at that time this region according to Reibisch would have been near the south pole. However such objections can always be met and conquered by a sufficiently ingenious advocate.

All hypotheses of polar migration require that there should be enormous changes of figure of the earth in order that the surface for every position of the axis should be in approximate equilibrium. These changes in the earth's body are supposed to take place isostatically, with only a moderate lag. There would be involved however a considerable stretching of those parts of the crust advancing toward the equator because of the greater equatorial circumference, compression in those parts approaching the poles. Several advocates have tried to read into the known crust movements an agreement with these requirements. But as many conflicts as agreements

could be cited, and it is not evident how sharply differential movements like the raising of the east African plateaus and the sinking of the Red Sea and Mediterranean basins can in any respect be responses to such a general change of figure.

The causes of the existence of Permian glaciation in low latitudes constitute one of the unsolved problems of geology, and the phenomena have been utilized by the various creators of hypotheses, but each hypothesis raises difficulties as great as those it is invoked to explain. Although the pole as located in the Permian by Kreichgauer would bring South Africa into the Antarctic circle, the Permian glaciation of Brazil and Australia would still be within the torrid zone. The Permo-Carboniferous axis as located by Jacobitti, while giving antipodal polar latitudes to northern South America and to Australia, would throw glacial South Africa into the torrid zone. The pendulation hypothesis of Reibisch, while permitting polar latitudes to invade Africa, would never give high latitudes to either India or South America.

The advocates of polar wandering have come near to agreement upon one supposition,—that in the Pleistocene the pole was in Greenland, or to the east of it, giving higher latitudes at that time to the glaciated regions of north-eastern North America and northwestern Europe. This would imply a polar movement of as much as  $15^\circ$  since the latter part of the Pleistocene. Reibisch in his first papers cites the fact that during the glacial period the volcanoes of equatorial Africa were glaciated to elevations 800 to 1,000 meters below the present limits. He regards this as a proof of his pendulation theory on the meridian  $E. 10^\circ$ , Africa then having a more northerly latitude. According to this, however, there should just as definitely have been no climatic change in the equatorial Andes, since these are adjacent to the oscillation pole. The fact that in Peru glaciation descended to altitudes below the present limits comparable to the descent on the equatorial mountains of Africa is, however, a most embarrassing fact not cited by Reibisch. For those who would move the Pleistocene pole

into Greenland, these facts of glacial advance in Peru beyond the present limits are even more disconcerting, since their position of the pole would bring Peru directly under the equator during the Pleistocene. Simroth, who goes far beyond Reibisch in his detailed discussion, does note and explain away these difficulties. He states (p. 533) that Reibisch had in a third, still unpublished work reached the important conclusion that a more northerly position of the Alps of only  $3^\circ$  or  $3\frac{1}{2}^\circ$  was necessary. The resulting elevation above sea level would be sufficient to originate the glaciation. In regard to the glaciation of the tropical mountains which according to others indicate a general lowering of terrestrial temperatures during the Pleistocene, Simroth says (p. 531):

Here it becomes our duty to go at least a little into argumentation. Kilimandjaro presents no difficulty. It lies so near the oscillation circle that pendulation could have easily carried it into other and cooler latitudes. During our Diluvium it must have lain well under the equator or somewhat north of it, but certainly not near either the north or south pole. One must, therefore, refer it back to the Tertiary in order that it should be permitted to wander to the south pole. There comes then the first thought in regard to those moraines; we do not know their age.

The problem as to how these moraines could have been preserved from erosion since the middle Tertiary is not entered upon by the author. Space forbids further quotation, but Simroth suggests as another alternative explanation that the glaciers may only appear to be far above their terminal moraines because visited in the dry season, during which a rapid melting takes place. As the moraines in question are stated elsewhere to be at elevations 800 to 1,000 meters below the present fronts of the glaciers, this would be a rapid seasonal melting indeed. His elimination of the difficulties connected with glaciation in the Andes is of a similar character.

In view of these quotations from Simroth it should be said that the great part of his work consists of a presentation of biological evidence. In this he is at home and his maps and text bring out many significant facts re-



garding the distribution of animals although some errors could be pointed out. The biologic evidence can, however, all be interpreted by other hypotheses than that of a polar pendulation.

The previous discussion has been given to show the vague and warped evidence upon which a system of terrestrial mechanics has been raised. But this is really not the way to test the hypotheses. They must stand or fall by the astronomical and mathematical implications. What then is the astronomic evidence?

Euler long since pointed out that in a rigid spheroid, if the axis of rotation did not exactly coincide with the axis of figure, the former would revolve around the latter. For the earth, if absolutely rigid, this revolution of the pole would be completed in 305 days. In 1890 Chandler showed that there was such a motion, but that the period was about 428 days. In 1892 Newcomb showed that the discrepancy between the calculated and the observed period was owing to the fact that the earth was not absolutely rigid. The difference in the period implied an elasticity of the earth's body comparable to steel, but did not show plasticity. The motions are confined within a circle about fifty feet in diameter. The actual path is not, however, a circle, and Chandler later showed that it was composed of two harmonic terms, the one about 430 days, the other 365 days. The former is the motion previously described, and is called the Eulerian nutation; the latter is regarded as due to seasonal changes in precipitation and in the seasonal shifting of atmospheric and oceanic currents. There is no suggestion of a third component of polar motion represented by a progressive shifting in one direction. Such a motion even if a fraction of a foot per year would have become evident owing to the length of the time over which refined latitude observations have been made. How does this observed fixity of the axis compare with the demands of the hypotheses of polar migration?

A movement of as much as  $10^\circ$  since the late Pleistocene, would apparently be at a much faster rate than the previous migra-

tions. Overlooking, however, this anomaly of changing rate, suppose the time to be as long as 200,000 years. This great length of time would minimize the annual rate, giving a movement of 18 feet per year. If the movement of the pole is reduced to  $3^\circ$ , as suggested by Reibisch in a later work mentioned by Simroth, this would be at an annual rate of 5.5 feet per year. The absence of even a small fraction of this motion within the period of precise astronomic observations would require the added supposition that progressive migration for some unknown reason had greatly slowed down or that pendulation was at its turning point. The astronomic evidence lends, therefore, no support whatever to the doctrine of a wandering pole.

Apparently Simroth thinks that the movement of precession involves a motion of the earth's axis within its body in a circle of more than  $20^\circ$  radius (pp. 534-536). This, according to him, is combined with the pendulation movement, the result being that the path of the pole is like the projected thread of a screw of which the axis is the meridian  $10^\circ$  E. In following out this idea under the title of the "Probable True Path of Pendulation" he naïvely says:

Possibly there speaks already in favor of a motion of the north pole in a screw line instead of a circle the uncertain statements of the handbooks. One reads now of 25,000, now of 28,000 years. I am not able to judge whence the different figures come. Do they not lie perhaps in the insecurity of the calculated elements which have been considered as circular arcs while they are in truth part of a screw line?

The final test of polar migration lies, however, in the mathematical analysis of the terrestrial motions. Mathematical astronomers have in general been opposed to the idea of a changing axis of rotation, the permanent fixity of the axis having been asserted by Laplace and many others since his day. This problem has been investigated further by Lord Kelvin, but, as previously stated, more especially by G. H. Darwin. The work of these men has been cited as offering no objection to a large or even indefinite wandering of the

pole. An examination of their original papers shows, however, that although they concede limited wandering to have possibly taken place under certain conditions, yet these conditions can not be admitted as existing throughout geologic time. Darwin's paper<sup>2</sup> is most thorough and conclusive. In it he shows that the axis of rotation will follow the axis of figure. That is, if profound subsidence of miles should take place at some locality, say Boston, and also at its antipodal point, until the connecting line was the shortest diameter of the earth and if there should simultaneously occur an upheaval around a great circle at ninety degrees from this point until this circle should constitute an equatorial bulge, then, and only then, could Boston come to lie on the axis of the earth.

If the change took place cataclysmically and the earth were sufficiently rigid, there would be set up a permanent Eulerian nutation, or circular wobbling of the pole, but if the change was slow and intermittent the Eulerian nutation would never be large. The lack of cumulative effect would be due to the variable positions of the instantaneous axis of rotation with respect to the principal axis of the earth at the times of successive impulses.<sup>3</sup> Thus, as a result of movements through the earth's body, shiftings of the axis of rotation would take place, keeping it close to the axis of figure. The axis of rotation at any time is consequently stable. To change it there must be shiftings of matter in order to change the axis of figure. As the radii would have to change in length by many miles for an extensive migration, the mere gradational processes of erosion and sedimentation could not be of much effect. There would have to be internal changes of form far greater than the known amounts of uplift and depression. Any explanation as to what force could cause the

<sup>2</sup> "On the Influence of Geological Changes on the Earth's Axis of Rotation," *Phil. Trans. Royal Soc.*, Part I., Vol. 167, 1877, pp. 271-312; Vol. III., Collected Works.

<sup>3</sup> G. H. Darwin, "On Professor Haughton's Estimate of Geological Time," *Proc. Royal Soc.*, XXVII., pp. 179-183, 1878.

earth to expand in one direction and contract in another direction to these great amounts is absent. Apparently the earth would have to be granted an amœboid power, which Simroth as the sponsor of the pendulation theory and a biologist might be willing to confer. By assuming a plastic earth and convective movements in its internal mass, energy could be supplied and a considerable polar wandering result, the process being analogous to a protoplasmic streaming. Lord Kelvin granted the possibility of a considerable polar wandering during the early plastic stage of the earth, but held that practical rigidity had prevailed throughout geologic history.<sup>4</sup> These statements are sufficient to show the conflict between the mechanics of a revolving solid globe and any hypothesis of unlimited wandering through geological time.

But movements of elevation have gone forward in some places, of subsidence in others. What maximum polar shifting could be the result of such continental and oceanic movements? Darwin has given a quantitative solution to this question. Taking the areas as in the most favorable situation to affect the axis of rotation, he assumes that one area is elevated 10,000 feet and another equal area subsides 10,000 feet. A table shows the relation between the size of these areas and the resulting deflection of the pole. A land mass as large as Africa thus favorably situated and undergoing reciprocal vertical movement with a section of oceanic bottom of like area, would result in a deflection of the pole amounting to about two degrees. If such changes were progressive and in the right direction Darwin states that they might account for a change of 10° to 15° since the consolidation of the earth. The kind of progressive changes which would account for this amount of shifting have not, however, been shown to have occurred through geologic history.

To affect the position of the pole the most favorable situation is for uplift to occur at two antipodal regions in latitude 45°; for depression to take place on the same meridian circle,

<sup>4</sup> *Trans. Geol. Soc. Glasgow*, Vol. XIV., p. 312, 1874.



but in the opposite quadrants from the uplifts. This amounts to a shifting of matter from two antipodal regions to regions  $90^\circ$  from them, but on the same meridian circle.

Erosion and sedimentation serve only to transfer sediment from the high parts of a continent to its low interior or its borders. The limestones may be partly deposited in other regions of the earth, but they constitute not over ten per cent. of the sediments. Isostatic readjustments would tend to affect the regions unbalanced by erosion and sedimentation. All of these actions have had but little tendency to shift matter from one octant of the earth's surface to another octant. Such surface processes have consequently had but little effect in shifting the poles.

The greater factor lies in the fragmentation of ancient continents, assuming that the possibility of this process be granted. But much of the Pacific must always have been a reservoir for the ocean waters. The fragmentation of Laurentia, extending the North Atlantic ocean basin, would largely be balanced against the sinking of Gondwana to form the South Atlantic. Downsinkings in the Indian Ocean and in the tropical Pacific would have but little effect since they lie mostly within the torrid zone. These down-sinkings, furthermore, need not have caused to bulge up by just that much some particular continent or continents. The up-swelling to compensate for the down-sinking may more readily be conceived as affecting the whole earth. Fragmentation, therefore, has not been areally distributed in such a manner as to produce the maximum effects calculated by Darwin as possible from vertical changes of 10,000 feet.

There is still another vital consideration, however. Darwin considers the case where elevation and subsidence is due to change of density, but not change of mass. Taking a superficial layer ten miles thick as not changing, but a swelling to occur throughout a section of crust from ten to fifty miles in depth, the change in the position of the axis would be but .0126 of what it would be if the uplift were due to an addition of matter. The pertinency of this is seen if it be noted that the

great plateau uplifts of the Tibetan region in Asia, of the Cordillera of North and South America, have been upraised with an approach to isostatic equilibrium from a state of low elevation and broad submergence in the early Tertiary. This is quite commonly viewed as the result of an intumescence in the crust beneath, due perhaps to the irruption of magmas and their accompanying heat and to the heat of orogenic deformation. But Darwin's figures show that uplifts due to this cause have a negligible effect upon the axis of rotation. Continental fragmentation and the sinking of Mediterranean basins, to such extent as they may have gone forward, may have been due to some contrary process of increasing density, the regional vertical movements thus conserving the isostatic principle.

From these considerations it is seen that closer examination tends to cut down more and more even those moderate limits of polar migration set by Darwin. It would appear that the assumption of polar wandering as a cause of climatic change and organic migrations is as gratuitous as an assumption of a changing earth orbit in defiance of the laws of celestial mechanics. Unless some wholly unsuspected forces are at work within the centrosphere, polar wandering has no more basis in science than Symmes' imaginings of a hollow earth. From all that is known at present the doctrine must be regarded as a vagrant speculation, not as a working hypothesis.

In closing this article it seems appropriate to indulge in a brief moralization. This paper does not contribute any new facts, but was written to show the untenableness of certain hypotheses, emanating in this instance from Germany and in danger of spreading in America, by confronting them with observed facts and mathematical demonstrations, much of which, originating in England and America, has been in the possession of science for more than a quarter of a century. Does not the history of this subject show the dangers of over-specialization within one division of science with the consequent putting forth of hypotheses regardless of the verdict of related

sciences? It certainly shows admirably the defects of the advocating method of research—the dangers of the ruling hypothesis. Probably also a more respectful reception has been given in this country to these hypotheses because they were voluminously presented in German and backed by the prestige of a German professorship, than if they had originated in this country. But if the writer is not mistaken, in Germany, preeminently the land of science, voluminous presentation is a fashion, and around the large body of high-grade work is a larger aureole of pseudo-science than is found in either England or America. We are sadly in need of knowing more German and in making larger use of foreign literature, but discrimination is necessary, and the writer is inclined to think that some Germans in turn might make larger use of scientific literature in the English language.

JOSEPH BARRELL

#### SCIENTIFIC NOTES AND NEWS

THE New Zealand meeting of the British Association has been abandoned. It will be remembered that a number of distinguished American men of science are on the way to attend the meeting as guests of the New Zealand government.

SIR ADOLPH ROUTHIER has been elected president of the Royal Society of Canada in succession to Professor Frank D. Adams.

THE commission authorized by the New York state legislature to undertake the scientific study of the causes of bovine tuberculosis, its economic and health effects upon the state, has been appointed by Governor Glynn. The members of the commission include: Dr. Th. Smith, director of the division of animal pathology, Rockefeller Institute; Dr. Hermann M. Biggs, commissioner of health, New York; Dr. Linsly P. Williams, deputy commissioner of health, New York; Dr. Philip Van Ingen, of the New York Milk Commission; Dr. Henry L. K. Shaw, professor of children's diseases, Albany Medical College; Seth Low, and Professor Veranus A. Moore, dean of the New York State Veterinary College, Cornell University.

THE Paris Academy of Sciences has awarded its La Caze prize of \$2,000 to Dr. Gley, professor at the Collège de France, for his works on physiology.

THE Sir Gilbert Blane medal of the Royal College of Surgeons of England has been awarded to Surgeon G. F. Syms, R.N.

DR. ALEXIS CARREL, of the Rockefeller Institute for Medical Research, has been made director of the Military Hospital at Lyons, throughout the war.

It is said that Dr. A. L. Skoog, professor of neurology in the University of Kansas, has been made temporary head of the La Petrie Hospital in Paris. Dr. Skoog was doing clinical work at the institution when the entire hospital staff was obliged to undertake military service.

DR. AUGUST LYDTIN, the author of important contributions to veterinary medicine and animal breeding in Germany, has celebrated his eightieth birthday.

THE first of the short addresses at the dedication of the new building of the Marine Biological Laboratory published in *SCIENCE* for August 14, should have been attributed to Professor Frank R. Lillie, director of the laboratory.

MR. C. A. McLENDON, botanist and plant pathologist of the Georgia Experiment Station, has accepted a position with the South Carolina Experiment Station as field pathologist. Mr. McLendon succeeds Mr. L. O. Watson who has gone to the Bureau of Plant Industry to take charge of the cotton wilt work in the south.

PROFESSOR CHARLES P. BERKEY, of the department of geology, Columbia University, accompanied by Dr. Clarence N. Fenner, of the Geophysical Laboratory, Washington, sailed from New York on August 15 for Porto Rico to make a geological reconnaissance of the island. This party represents the New York Academy of Sciences, which has undertaken, in connection with the government of Porto Rico, a complete natural history survey of the island. It is hoped during the present season



to determine the fundamental geological formations with their larger structural relations, and reveal the problems that additional parties are to investigate in succeeding seasons.

MISS ALICE EASTWOOD, curator of the botanical department of the California Academy of Sciences, has recently returned from a collecting trip to Dawson, Yukon Territory, Canada. In order to be on hand for the earliest vegetation, particularly the willows, Miss Eastwood left San Francisco on April 4. The journey from Whitehorse to Dawson, a distance of over three hundred miles, was made in an open stage on runners over the snow and the frozen rivers. Full material was obtained of all the willows from the winter stage in some species to the fruiting stage in all, with leaf specimens from the flowering bushes. Eleven species were found at Dawson within the town limits, and four were added from the higher mountains of the Yukon near Dawson. The return trip was made up the river to Whitehorse and every day opportunities for collecting were afforded when the boat stopped to take on wood. Collections were made at Whitehorse, Atlin, Llewellyn Glacier, Lake Bennet, from Log Cabin to White Pass and Skagway. Small collections were also made on the way to Seattle when the boat stopped at Sitka, Wrangell and Killisnoo. The trip was made at the instigation of Professor C. S. Sargent, head of the Arnold Arboretum and through the co-operation of that institution and the California Academy of Sciences.

MR. ALFRED JOHN JUKES-BROWN, F.R.S., lately of the English Geological Survey, died on August 14 at the age of sixty-three years.

MRS. MARY A. ALBERTSON died on August 19, at the Nantucket Maria Mitchell Memorial, where she had been librarian and curator for ten years. To her much of the success of the memorial is due. While the astronomical work and the observatory received her faithful attention, she early organized a botanical department. Having been associated with Professor Mitchell in earlier days she knew her great love for flowers and worked to collect a complete herbarium of Nantucket flora (native and introduced). It is gratifying to

report that she lived to see this nearly completed.

THE U. S. Civil Service Commission announces an examination for chief petroleum technologist to fill a vacancy in this position in the Bureau of Mines, for service in San Francisco, Cal., at a salary of \$4,800 a year. The duties of this position will be to supervise and participate in the technologic and other scientific and economic work of the Bureau of Mines in relation to petroleum and natural gas, as to production (which involves a consideration of the oil- and gas-bearing strata), storage, transportation and refining; the prevention of waste; the prevention of loss from underground water encroachment and other economic problems affecting the industry. Graduation with a bachelor's degree from a college or university of recognized standing, special or graduate work in practical geology, and not less than five years' responsible experience in various practical petroleum operations, such as would fit the candidate for the above enumerated duties, are prerequisites for consideration for this position. This examination is open to all men who are citizens of the United States and who meet the requirements.

NOTICE is given by the organizing committee of the Nineteenth International Congress of Americanists that the session which was to be held in Washington from October 5 to 10 of this year has been postponed on account of the European war. An expression of opinion was asked of the membership, which has already reached the exceptional number of three hundred, and the almost unanimous reply was to the effect that since the many European members and governmental delegates could not attend, it would be impolitic to hold the meeting during the present season. A new date for the session will be decided upon as soon as conditions permit. It is suggested that by putting off the congress till the summer of 1915 arrangements may be made to hold a joint meeting with the Pan-American Scientific Congress, which is to meet in Washington next season. This would have the great advantage of enabling foreign members to

attend both congresses and at the same time to visit the two California Expositions.

THE British Iron and Steel Institute has been obliged to abandon the holding of its proposed autumn meeting in Paris.

IN consequence of the war, the publication of the British Pharmacopœia for 1914 has been postponed.

THE State Geological Survey under the direction of Professor Russell D. George has completed a series of contour and topographic maps of Colorado which have been placed in every library and school in the State.

MESSRS. WILLIAM WESLEY and Son, London, have in view of the Napier tercentenary issued a catalogue of astronomical, mathematical and other tables. This catalogue includes upwards of 300 volumes, published from the middle of the fifteenth century to the present time.

THE European war has for the present, at least, totally closed the European market to American radium ores. As is well known, the uranium ores of Colorado and Utah are sold exclusively for their radium content, so little use being known for the uranium that the ores can not be sold for their content of that element. The closure of the European market leaves but one known buyer, so that while the war lasts and probably for some time afterwards the market will be restricted and without the benefit of competition. Had the bills introduced in Congress been passed, the United States government would probably also have been in the market as a buyer, and the miner might have had at least the choice between two purchasers.

THE Bureau of Standards, Department of Commerce, has published a circular containing suggestions as to location and equipment of gas testing laboratories, a description of some of the accepted forms of apparatus, directions for the making of the various tests, and recommendations as to the interpretation of experimental results. It does not discuss the testing work necessary for good works control; it deals rather with methods which are intended for use in city or state official testing or in works laboratories which are checked

by city or state inspectors. No attempt is made to fix on a single method to be used in every case, for it is not believed that uniformity of method is always necessary in order that the results of tests be considered standard. In each case as much freedom in choice of method is allowed as seems permissible; but the simplest procedure or apparatus with which satisfactory results can be had is given preference. Great advantage will result to companies and workmen alike by the general adoption by the several states of a single standard set of safety rules, which can be revised in accordance with the progress of the art and the combined experience of all the companies and commissions of the country. Thus will every state and every company secure the advantage of the experience of all. What particular rules do not apply their omission will of course cause no conflict in practise. If it is necessary for any state commission to adopt additional rules, that could be done at any time by special orders. This would be easier and less confusing than to have a different set of rules for each separate state. Acknowledgment is made of the cooperation by national associations, state commissions, company officials, and individuals. The conclusions reached by the Bureau of Standards from the combined experience of many of the most experienced companies and individual engineers and a thorough study of a large amount of literature and statistics are now offered with the hope that they will constitute a substantial contribution to the widely evidenced public need for a standard set of safety rules. It is believed that a material reduction in present life hazards to electrical workers may be realized by the general adoption and use of these rules. The study of life and property hazards incident to the generation, distribution and use of electrical energy includes the consideration of both construction methods and operating practise. Analysis of the available data on electrical accidents demonstrates their preventability in very large proportion by use of definite operating precautions. This is especially true with those accidents occurring to workmen engaged in electrical work. Rules



for construction, installation and maintenance of electrical equipment to safeguard employees and the public are now under preparation by the Bureau of Standards, Department of Commerce. The rules for safety in the operation and handling of electrical lines and equipment, just published, proceed from a painstaking study by the engineers of the bureau of existing rules and practises. These are found to vary widely and to offer a very unsatisfactory basis for the formulation of mandatory codes by any state commission, unless a very extended study is made and the combined experience of many companies and workmen utilized. Many existing sets of rules have been developed from insufficient data and experience, while the vast majority of companies have no rules whatever in effect. This lack of rules in force is partly due to inaction on the part of state authorities and partly to the difficulty and expense each company encounters in preparing its own rules in any adequate form. The assistance of state commissions, operating companies and electrical workmen has been freely given to the bureau in this work, and the rules in their present form are offered to the public for criticism, discussion, and, so far as may be found desirable, for general adoption. The scope of the safety rules includes all operation of and work on or about power and signal lines, and the electrical equipment of central stations, substations, mines and testing departments. The rules are divided into three parts. The first two parts consist of general rules which apply to the employer and to the employee, respectively, and the third part comprises, under separate headings, those special rules which apply particularly to employees engaged in special classes of electrical work.

THE U. S. National Museum announces that it is exhibiting some designs in silk dress goods which use the designs and symbols left by the Aztecs and other early Indian peoples. Much material for designs pertaining to this early period of American history was available; buildings, temples, monuments, pottery, basketry and blankets are covered with picture-writings which form artistic designs. Not

only the designs proper were adaptable but the colors as well, a fact which has materially assisted in the creation of these new American fashion designs. The textile division of the museum has installed a series of pure dye taffeta silks, contributed by the manufacturers, which show the reproductions of these ancient Mexican designs printed on soft clinging fabric. The designs comprise the Aztec moon in rainbow tones on blue and taupe; the Aztec armadillo and arrow pattern in colors on peacock-blue; Korteze—an Aztec hieroglyph—on dark green and satin-striped white taffeta; the Aztec coat-of-arms on navy blue, and an all-over design of Mexican feathers in shades of blue, green and brown. Other designs are reminders of the Pueblo Indians, one consisting of a rattlesnake symbol printed on Indian red, while another resembles a Navajo rug in which zig-zag stripes and a diamond arrangement of figures appear.

#### UNIVERSITY AND EDUCATIONAL NEWS

It is announced that the British universities will open as usual in the autumn. The Rhodes scholars from the United States and from the British colonies are expected to be in attendance at Oxford.

THE Nantucket Maria Mitchell Association is endeavoring to collect \$12,000 to endow an astronomical fellowship at Harvard College Observatory. Upwards of a thousand dollars have been given for this purpose, and in addition Dr. E. C. Pickering, the director of the observatory, and Mr. Charles S. Hinchman, of Philadelphia, have each subscribed \$250 for the inauguration of the fellowship.

DR. D. A. CAMPBELL, of Halifax, has promised \$60,000 to endow a chair of anatomy at Dalhousie University, Halifax, in memory of his son, the late Dr. George Campbell.

GEORGE PEABODY COLLEGE for Teachers has now an endowment of \$3,200,000 of which \$2,000,000 is to be used as a permanent endowment. Part of the remaining \$1,200,000 is being spent on new buildings. The Household Arts building and the Industrial Arts building have already been completed and this year

housed the summer school of a thousand students. In the basement of the latter building is located the power plant for heating, lighting and ventilating the buildings over the entire campus of 50 acres. Two other buildings are in process of erection. One is the Jesup Psychology Laboratory costing \$75,000. The other is the Social Religious building, which is designed to play an important part in the life of students, both in a social way and as a preparation for real service in life. This building will be the most commodious on the campus and will probably cost about \$300,000.

MR. DORR SKEELS, of the U. S. Forest Service, has been elected dean of the new school of forestry that has been established at the University of Montana.

DR. THEODORE C. FRYE, professor of botany, has been named temporary dean of the college of science by the University of Washington regents to succeed Dr. Henry Landes, acting president of the university.

THE following promotions have been made at the University of Colorado: Ralph D. Crawford, Ph.D., to be professor of mineralogy and petrology; Max M. Ellis, Ph.D., to be assistant professor of biology; Frank S. Bauer, B.S., to be assistant professor of mechanical engineering. The following new appointments for the coming year have been made: James L. Merrill, B.S., instructor in engineering drawing; Walter F. Mallory, B.S., instructor in mechanical engineering; Clarence L. Eckel, B.S., instructor in civil engineering; Edward R. Mugrage, M.D., instructor in pathology; Jay W. Woodrow, Oxford University Rhodes Scholar, 1910-12, Ph.D. (Yale, '13), instructor in physics; Esbon Y. Titus, B.A., instructor in chemistry.

#### DISCUSSION AND CORRESPONDENCE

##### COMPOSITION AND THOUGHT

TO THE EDITOR OF SCIENCE: In the February issue of *Modern Language Notes* appears from the hand of Professor French a rather unappreciative review of a new type of rhetoric by Steeves and Ristine; the title of the work is "Representative Essays in Modern Thought."

The review may go far to discourage the use of the book. And, since I doubt whether many of the readers of SCIENCE realize the importance to them of this innovation in rhetorical fields, I beg indulgence to comment upon the method by which the new rhetoric has been used in a western university.

"Representative Essays in Modern Thought" is intended to serve a new purpose in the rhetorical kingdom; students already trained in the essentials of expression are here presented with essays by Mill, Huxley, James, Maine, Clark and other writers famous not only for the clearness of their expression, but also for the solidity and pregnancy of their material. The student, having read any given essay, is asked each week to present his reaction upon that essay. Needing no discussion, surely, are the value of the analysis and outlining of these essays, and the mere advantage of the incidental knowledge gained. But two other points may well be emphasized: the awakening of the promising student to a genuine understanding of the timidity and slovenliness of his habits of thought; and the placing before him in the second semester of his freshman year at college of the sound principles of topics he hears everywhere discussed.

In the second semester of his freshman year, I repeat. That is the point which needs defense against the avowed antagonism of more than one instructor of rhetoric. The students in our modern universities who most need to learn to write are not those who already love to write; rather, they are the students in science, engineering, law and other professional fields. Yet it is perfectly obvious that our crowded curricula seldom, if ever, allow these students to take advanced courses in composition. Nor, be it predicated at once, would I rush the honest journeymen in such courses into the study of Steeves and Ristine. How much could be done for the mediocre student I am rather uncertain; and I refrain from the speculation in futurities in which even my scientific friends are prone to indulge. Here, statements are limited to what can be done for second-semester freshmen who have



finished the routine of the first semester with distinguished grades—at my particular university, grades of *B +* or above, on a scale of *A, B, C, D, E*.

Such freshmen, then, are segregated in a special section, the purpose of which is carefully explained to them beforehand, and for which, indeed, they have been encouraged to work from the time their ability was discovered; clever, "literary" writers are designedly eliminated, and pressure to enter the section is brought to bear upon students in science, engineering and law. Let me note in passing that few girls elect the course—at least, as yet. The weekly papers that are written are from three to six pages in length; their nature can best be indicated by presenting some of the topics actually written upon. I doubt exceedingly whether either expert or laymen would question the value of the topics; the expert will rightly question, *a priori*, the ability of a mere rhetoric instructor to criticize the themes.

Mill—"On the Liberty of Thought and Discussion."

Would Mill Accept a Position on the Board of Censors for American Papers?

Mill and the Suppression of the *Cosmopolitan*.

Mill and the Study of Sex Hygiene in High School.

What are Truth and Error?

Are Christian Missionaries Persecutors of Freedom of Thought?

Does Mathematical Truth Differ from Ethical?

Mr. Roosevelt and Some of his Assumptions of Infallibility.

Morley—"On the Possible Utility of Error."

The Effect on Mankind of Sudden, Supreme, Universal Conviction that There Is No God of any Kind (Use method of classification).

Should Children Read Fairy-tales?

Were An Absolute Cure for Vicious Diseases to be Discovered, Should the "Truth" be Spread?

A Half-truth of Modern Science.

Huxley—"Darwin on the Origin of Species."

The Evidence of Hybridization—Does it Support Darwin to-day?

The *Archæopteryx*—its Relation to the *Pterosaur* and the *Compsognathus* as a Proof of Evolution—of Darwinism?

The Electric Fishes—How have the Neo-Darwinians Met the Problem?

Is a Darwinian an Atheist?

Is there a Fallacy in the Syllogism upon which the Discrimination of Species from Varieties Depends?

Some Theoretical Objections to the Darwinian Explanation of Secondary Sexual Characteristics.

"Ponderous topics for a rhetoric Ph.D. to pass judgment upon?" Yes, my dear scientist or political economist, I echo the satire—the more so because, in my own case, I was reluctantly led to do much graduate work in various remote fields of literature. Still, though rhetoric instructors are poorly prepared to teach sensible courses in composition, the matter is not so bad as it appears on the surface. If the captious critic will examine the topics given, he will note that they fall into two distinct groups: one type of subject may be written upon without research; the other certainly requires special knowledge. Surely, in watching a student detect logical fallacies in Morley or Huxley, the rhetoric instructor is at home; he has long taught argumentation. The research topics the "canny" instructor can easily limit to his own immediate knowledge. *E. g.*, from books and from colleagues one can gather information concerning the archæopteryx, the eohippus or the amphioxus; and no rhetoric instructor need despair of grasping the essentials of the planetesimal hypothesis or the theory of mutations. For distinctly personal reasons, I should not this year allow a student to write upon the effect the discovery of radium had upon any given detail of the atomic theory; next year I may even have apprehended a little on that subject. Moreover, let it be instantly admitted, this course in modern thought is essentially a course in logic and composition; I am interested in using science or political economy only because it affords resistant material to set the freshman's teeth in. What he is to detect is that Darwinism proper is as free from athe-

<sup>3</sup> Particularly Planck's Rectorial address in the current (July) number.

istic implications as the orthogenesisists claim to be from neo-vitalistic stigmata; that Socialists of the type of Hillquit are not anarchists and that a very pretty fallacy underlies the assertion that in the Socialistic state all incentive to invention will vanish; that one can scarcely be at the same time a neo-Kantian and a scientific ethicist. What is further aimed at is to teach the scientific or engineering freshman whom nature has endowed with brains the ability to express his inductions or deductions in readable terms—to, well, let me suggest, write upon Mendelism after the *rhetorical method* of Punnett, and not after that of —. The blank is not hard to fill. If scientists are ever to slay the religion which Huxley likened to Bourbonism, they must be capable of approaching the public with other explanations of abstruse matter than such mathematical exposition as even Professor Bateson admits he “could not follow.”

And at this point I verge on my final plea for the use by instructors of rhetoric of some such book as Steeves and Ristine. With all humility and yet all firmness, I contend that the proper teacher of such courses is not the ordinary composition instructor, aided by casual, if expert, colleagues from the other schools, nor, above all, the man with training narrowly limited to science, engineering, or law, but the rhetoric instructor who is wise enough to assign only such topics as he himself has taken the trouble to master. Why not the ardent young scientist? Because the very reason for rhetoricians adopting the new text is that they may train the scientists of the next generation to learn to use the language that seemed adequate to Darwin and Huxley, Smith and Galton, Tyndall and Faraday. I rather suspect that a certain professor of physics was not entirely alone when he so surprisingly confessed in the preface to his well-known book that “he trusted he had made no more errors than he had hoped for.” There is, however, a further reason for the objection to turning such courses over to scientists. Scientists love theories and even hypotheses: witness the pleasing manner in

which Eimer flayed Nägeli for approximating neo-vitalism—and then note how charmingly mystical is Eimer's own analysis of orthogenetic forces. The basic thing in these thought courses is that there be no adherent to this school or that supervising the course. For, whenever the mere imparting of information or speculation is allowed to take the place of the study of coherent arrangement of material and sharp criticism of independent thought, then the chief value of such courses is thoroughly vitiated. And yet, if rhetoric instructors do not awake, some time or other scientists, engineers and lawyers will somehow face the problem of themselves instilling the principles of unity and coherence into their promising students.

MIDDLE WEST

#### SCIENTIFIC BOOKS

*Problems of Science.* By FEDERICO ENRIQUES.

Authorized translation by KATHARINE ROYCE, with an introductory note by JOSIAH ROYCE, Professor of History of Philosophy at Harvard University. Chicago, The Open Court Publishing Company. 1914. Pp. xvi + 392.

Among mathematicians Enriques, who is professor of projective and descriptive geometry in the University of Bologna, has long been favorably known for his contributions to geometry, especially for his admirable treatise on “Projective Geometry” and for his penetrating essays on “The Foundations of Geometry.” In the work before us the distinguished geometrician addresses a far wider circle of students and thinkers: not only mathematicians, but psychologists, logicians, philosophers, astronomers, mechanicians, physicists, chemists, biologists and others. For the discussion, which is as wide-ranging as the philosophic writings of Henri Poincaré or as that of John Theodore Merz in the first two volumes of his “History of European Thought in the Nineteenth Century,” deals with fundamental questions drawn from every large department of modern science.

The original text, “*Problemi della Scienza*,” was published in 1906 and has since appeared in German and French translations. Many a



student will feel grateful to the translator and the publisher who have made the work accessible in good form to those whose reading is necessarily confined to the English language.

The work is, in the best sense of the term, a philosophical work. Accordingly, one can not but wonder a little why the author did not choose to call it "Philosophy of Science" instead of "Problems of Science." Perhaps the decisive consideration was similar to that which led Messrs. Whitehead and Russell to entitle their great treatise "Principia Mathematica" instead of "Principles of Mathematics": they feared the warmer title might attract many readers incompetent to understand the work. Doubtless Professor Enriques desired his work to engage the attention of men of science, and he may have reflected that most of these gentlemen are rather repelled than attracted by titles in which the word philosophy occurs. Is our author himself a member of this majority? His evident great care not to be fooled by words or to be lost in nebulous generalities seems to indicate that he is. Confirmatory indicia are to be found in some passages of the work. It is essential "to eliminate all transcendental processes of definition and of reasoning," says Cesaro in the beginning of his lectures on the infinitesimal calculus. Enriques quotes those words of his fellow-countryman and heartily approves them (p. 16) as designed to warn the student "to banish from his mind all metaphysical ideas" ! Again, p. 31: "Metaphysics not only puts together symbols without sense, but," and so on. Again, p. 208: "And precisely to ignorance of this subject (modern geometry) are due those strange conclusions over which some philosophers are still toiling." Once more, p. 308: "But even if these objections were not manifest, of what use is it to confute a philosopher? Schopenhauer said nothing could be easier or more useless." Just why the testimony of Schopenhauer is adduced is not quite evident unless it be on the principle that it takes a philosopher to catch a philosopher. One who has attended meetings of philosophic associations and meetings of scientific associations can scarcely have failed to notice this very

significant difference: at a meeting of scientific men, when a paper is presented, the author's colleagues assume that the author has probably made a contribution of some value and that it is their privilege and duty to understand it and sooner or later to estimate it; at a meeting of philosophers, when a paper is presented, the author's colleagues usually proceed at once to discuss it with the air of "of course the author's contentions are erroneous and it is our privilege and pleasure to show that they won't bear criticism."

That Professor Enriques should not wish to pose as a philosopher as distinguished from the character of man of science is indeed entirely understandable. Yet his work is a very important contribution to the philosophy, the methodology, the epistemology of science, and, whether or not he would own it, he has shown himself to be a philosophic thinker of immense learning and of great power both critical and constructive. But what kind of philosopher is he? To what school does he belong? Is he a realist or an idealist or a rationalist or a pragmatist or an empiricist or a positivist or some other variety? The answer is that he is at once all and none of these things. He is too big to belong to any of the schools. His thought goes crashing into and through all of them, and, when he has passed along, the scholastic architectures look much as if they had been struck by a discourse of Henri Poincaré. One can not paste a label on Enriques and then inform people of his doctrine by pointing to the label. The only way to ascertain what his doctrine is is to read and ponder what he has said. But who *can* read it? Not many know enough to read it all, but there are many qualified to read it in part, some this part, some that, some another. Even historians (whose province includes the whole activity of man and nature) might try it; so might sociologists, lawyers and men of letters. Should they fail to understand it—well, the consciousness of one's limitations is not always unwholesome, and if it become unbearable, one can take refuge in the soothing reflection that it was Leibnitz who was "the last of the universals."

The author's aim is to contribute to the advancement of epistemology. It is not, however, epistemology in the Hegelian sense. For Enriques, epistemology has for its object "to explain the process by which the most advanced science is built up." It is, he says, "of the first importance that epistemology should be conceived as an actual positive science"; a science in the making, he, of course, means, as is abundantly evident. In a word, epistemology is to be conceived as the science of knowledge, and no one knows better than our author that to make a contribution to the science of knowledge demands knowledge of science. He would probably not deny that, as Thomas De Quincy so well said, every problem of science ultimately roots in metaphysics. But he is convinced that it is not therefore necessary or profitable to be always burrowing like a mole in the black soil where the roots are hid. Bergson the book does not know, probably because the Frenchman's splendid star had not yet risen when the book was written. Doubtless he would agree with Bergson that after the method of science has said all it can of a given object there remains in it an untouched residuum—something of which it is possible and desirable to gain that kind of knowledge that one means when, for example, one says of one's self: I know how to move my arm. Perhaps the Italian would agree with the Frenchman that there is thus indicated a proper province and task for metaphysics, namely, the province and task of winning that residual kind of knowledge through a kind of "intellectual sympathy" with the object, through a kind of fellow feeling with it. But the Italian's epistemology is a different sort. It is "positive" epistemology. It has "a real object to explain." This object is the upbuilding of what we call scientific knowledge and so it has "actual problems to solve." These "ought not to depend upon the inconstant opinions of philosophers" nor "upon the social interests that determine these opinions." Epistemology becomes "positive" only in so far as it is established "independently of metaphysics." For Enriques the supreme

desideratum in this enterprise is "systematically to banish whatever pertains to the transcendental process of the reason."

What is this dread process? It shows itself in many guises, most commonly, perhaps always in last analysis, as a subtle assumption that an infinite series has in some way a final term, or, if not a final term, at all events an actual limit. In this way all sorts of absolutes, absolute motion, absolute substance, absolute time, absolute morality, and so on, come to figure in our thinking. Such absolutes may have emotional value and so constitute "a problem for the psychologist" but as concepts for scientific use they are worse than worthless. We can not even show that an infinite sequence has a limit by merely showing that it neither diverges nor oscillates.

One of the best sections of the introductory chapter is that in which is discussed the question of "so-called insoluble problems." It is contended that "in a broad sense there are no insoluble problems." "There are only problems not yet suitably stated." Some one ought to write a work on the history of curiosity. Why have questions arisen in the order in which they have arisen instead of some other order among an infinite variety of thinkable orders? Why have questions seemed to be questions when they have really not been questions? Our author's thesis respecting insoluble problems is well illustrated by him in connection with an admirable account of the famous so-called problems of squaring the circle, perpetual motion and alchemy. This chapter is mainly concerned, however, with the distinction between subjective and objective in scientific knowledge. It is argued that both kinds of elements enter into all scientific knowledge, but as such knowledge advances the subjective component tends to disappear and the objective comes to be more and more. In fact, the two elements "are not two irreducible terms of knowledge, but they are rather two aspects" of it. The question is considered in relation to measurement and to scientific construction. This leads to a critique of positivism in relation to metaphysics, to physics, to biology, to psychology, to history



and to sociology. The entire critique, in which the doctrine of Comte is carefully appraised, hinges on the proposition that, "Strictly speaking, a theory can not be called positive, unless it consists purely of verifiable hypotheses." Those who hope that psychological problems will ultimately receive physiological solutions are not encouraged. The same may be said of those who seek an exclusively economic explanation of the facts of history.

The second chapter (of nearly 50 pages), which deals with "facts and theories," opens with a discussion of dreams and reality. What is reality? What is its criterion? To make a genuine contribution to the literature of that hoary question is something of an achievement. Enriques has made such a contribution. The conclusion is that "the true characteristic of reality is the correspondence of the sensations with the expectation." Reality is thus defined as an invariant, a mathematical term that is gaining currency in various branches of natural science. "There are certain fixed groupings, independent of us, among our actual or supposed volitions on the one hand, and the sensations produced by them on the other. These groupings correspond to what we call the real." The real thus is "an invariant in the correspondence between volition and sensation." The definition involves a hypothetical element: it is presupposed that actual sensations would recur if their conditions were reproduced; but such reproduction is frequently impossible. This conception of reality is examined in relation to the past, to psychology, to society, to biology, to physics, to astronomy and so on. What of hallucinations? The problem is frankly recognized but no pretense of a solution is made. A valuable suggestion, however, is offered. It is that "the patients are unable to doubt and so submit their false impressions to a critical proof directed by the will." The object of an hallucination is unreal because the subject's deception is real. How does knowledge pass from common facts to scientific facts? The answer is: by passing from the subjective or individual view to the objec-

tive or social view, from the personal to the impersonal view. A common fact is a fact viewed in relation to the beholder; a scientific fact is a fact viewed in relation to surrounding facts. "If I strike a copper plate with a hammer, the plate grows hot," is a common fact. "Bodies are heated by percussion" is a scientific fact. Thus the conception of scientific fact merges into that of law. What is the relation of hypothesis to scientific knowledge or knowledge of reality? "To make an hypothesis signifies: (1) to expect or to foresee given sensations under certain future conditions; (2) to arrange among the groups of actual or controllable sensations, an intermediate grouping which shall serve to associate them in a given order of prevision." This view of the function of hypothesis is elaborated very instructively in connection with such topics as the value of scientific knowledge; knowledge by means of concepts, empiricism and rationalism, the acquisition of knowledge, scientific theories, the theory of gravitation, the electrostatic theory of Poisson, the theory of solutions and the economy and the psychological development of theories.

This many-sided critique of the scientific rôle of hypothesis leads naturally to the question of the offices of induction and deduction in epistemology, and the third chapter (72 pages) is accordingly devoted to problems of logic. To the oft-repeated stupid charge that formal reasoning can not lead to gain of knowledge, our author justly replies that such reasoning serves as an instrument of transformation which, though it does not alter the conceptual data of knowledge, but leaves their truth or falsity to be shown by other means, yet establishes a connection whereby the truth or falsity of certain data implies the truth or falsity of other data. For example, formal logic may show that an hypothesis  $H$  implies a consequence  $C$ , and it often happens that we can test  $C$  directly and thus test  $H$  indirectly. The work of induction and deduction is teamwork. Science can not dispense with either of them. The importance of modern developments in symbolic logic is recognized. An exceedingly valuable discussion of the nature,

function and varieties of definition is given. Every college, and especially every university, ought to give a course of lectures on the subject of definition. There is scarcely any other important scientific subject of universal interest respecting which educated people know so little, but they are not aware of it. How does abstract logic get applied to reality and what are the limits of such application? This very difficult question is examined under many aspects and in many concrete connections: logical representation and the postulate of knowledge, substance (matter and energy), cause, actual value of logical principles, the value of logical principles, the objective reality of logic, the problem of verification, the verification of explicit hypotheses, the experience of a finite number of objects, experience of the continuous, the postulate of continuity and the psychological representation of cause (why and how), the confirmation and verification of implicit hypotheses, the present crisis in political economy, the vicious circle in science and the physiological aspect of logic.

There follows a chapter (59 pages) devoted to geometry. Geometry is viewed, on the one hand, as a part of physics, and, on the other hand, as a purely abstract science. In the latter sense it is a prolongation of logic. Perhaps the most striking thesis in a thoroughly up-to-date discussion, rich in suggestions and insights, is found in that section which deals with the parallel between the historical development and the psycho-genetic development of the postulates of geometry. The thesis is: "*The three groups of ideas that are connected with the concepts that serve as a basis for the theory of the continuum (Analysis situs), of metrical, and of projective geometry, may be connected, as to their psychological origin, with three groups of sensations: with the general tactile-muscular sensations, with those of special touch, and with those of sight, respectively.*" There be psychologists, and some educators, who think mathematics is so detached from reality as to be an inferior discipline. We should be much interested if these gentlemen would favor us with an expert opinion regarding that thesis of Professor Enriques.

A chapter of 64 pages on mechanics regarded as an extension of geometry is followed by a final chapter of 88 pages on physics in which the leading question concerns the extent in which physics may be regarded as an extension of mechanics. An admirable review and critique of the conceptions and principles of classical mechanics and classical physics in their relation to the new more or less speculative ideas lead to the general conclusion: "*Physics, instead of affording a more precise verification of the classic mechanics, leads rather to a correction of the latter science, taken apriori as rigid.*" The wide range of the author's interest and thought is specially indicated by the closing pages, which are devoted to the mechanical hypothesis and the phenomena of life. The conclusion is that, "in the actual state of our knowledge, *the mechanical hypothesis does not appear to be incompatible with the phenomena of life, but it is unimportant for the study of these phenomena.*" The student will find it instructive to compare the conclusion and the temper of the related discussion with the temper and conclusion in Dr. Crile's "A Mechanistic View of Psychology," published in *SCIENCE*, August 29, 1913. In this connection one should consider an article by Professor W. B. Smith, entitled, "Are Motions Emotions?" published in the *Tulane Graduates' Magazine* for January, 1914. An even more significant deliverance by the last-named author dealing with the claims and limitations of the mechanical hypothesis is an article bearing the title "Push or Pull?" published in the *Monist*, January, 1913.

In a review of moderate length it is not possible to give an adequate account of Enriques's book. We know of no other work that gives so keen a sense of the unity of all branches of science. A final word as to its manner. The section headings are too numerous, breaking the continuity of the reader's attention; and there are some obscure sentences and paragraphs. These are external faults and are trivial in relation to the inner excellencies of the work.

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*X-Rays. An Introduction to the Study of Röntgen Rays.* By G. W. C. KAYE. Longmans, Green & Co.

Since 1895, when Röntgen made his epoch-making discovery of the X-rays, an immense amount of research work, experimental and theoretical, has been done on their properties. This work has produced a remarkable series of discoveries of high interest and fundamental importance. A connected account of the latest results in this branch of physics by one who has made several important contributions to it can not fail to be welcome. Dr. Kaye has succeeded in producing a very useful summary of the latest results together with a brief account of the historical development of the subject and numerous practical details which should be useful to any one working with X-rays. The book contains a number of excellent illustrations.

A few minor errors have crept in; for example, on page 148 it is stated that the total ionization produced by a beam of homogeneous corpuscular rays is independent of the velocity of the corpuscles, which is obviously absurd.

Chapter XII. contains a clear account of the recent work, initiated by Lane, on the diffraction and reflection of X-rays by crystals, which has established the theory that X-rays are merely light rays of very short wave length.

Chapter XIII. contains a discussion of the various theories of X-rays which have been put forward. The problem which remains to be solved is the emission of high velocity electrons by matter when exposed to X-rays.

H. A. WILSON

*Irritability, a Physiological Analysis of the General Effect of Stimuli in Living Substance.* By MAX VERWORN, M.D., Ph.D. New Haven, Yale University Press, 1913.

To the physiologist who wishes, for the clearing of his vision, to return from time to time to a consideration of the fundamentals of his science, no better opportunity can be offered than that contained in the published volume of lectures on irritability by Professor Max

Verworn. The biologist, too, will find in its pages an unusually rich presentation of the facts of cell behavior, interwoven, correlated and interpreted, with meanings that separately they fail to convey.

The book, a re-writing of the Silliman Lectures of 1911, is a philosophical treatment of the nature of irritability as one of the general manifestations of living material, followed by a study of the laws and effects of stimulation, undertaken for the light that such knowledge may throw on the nature of the vital processes of which irritability is a manifestation. Its facts are drawn from the results obtained during twenty years consistently devoted to the problem by Verworn and his pupils, and from the work of others in the same field. The importance of its conclusions may be estimated from the breadth of its experimental foundations.

The opening chapter gives a careful review of the historical development of our modern ideas of the subject, from the first generalizations of Glisson, with whom originated the "doctrine of irritability," down to Virchow's conclusion that nutritive, functional and formative reactions of cells are the basis alike of normal and pathological manifestations in cell activity. The importance of Virchow's teachings in the modern interpretation of diseased conditions has perhaps overshadowed their equally great importance to general physiology: indeed, these, with inhibition (Weber) and narcosis (Claude Bernard), may be looked upon as the starting-point of Verworn's philosophy.

The second lecture, on the nature of stimulation, is perhaps the most striking, containing as it does clearer definitions of the meaning of the words stimulation and irritability than we have had heretofore, and leading to a better understanding of the scope of the unsolved problem of the nature of the vital processes. Beginning with a lucid presentation of the difficulty in differentiating between the "cause," so-called, and the "conditions" of a biological, or indeed of any, process, it is pointed out that "all conditions for a state or process are of equal value for its existence, for

they are all necessary." And though we are prone to consider the last determining condition for a process as its cause, in reality "a state or process is solely determined by the sum total of its conditions." Living material by virtue of its irritability adapts itself to changes in the conditions of its existence, by various manifestations with which we are familiar. "Life is the entire sum of the vital conditions."

A stimulus is every alteration in the vital conditions, being a stimulus only when considered in relation to the previously existing state. The alteration may be subliminal, minimal, submaximal, or maximal; it may be harmless or injurious; short, long or the initiation of a new condition which is to persist. Since there are certain internal vital conditions that are always undergoing change, as in development, and external vital conditions that may exist unchanged, and independent of the vital process, a suggestion is made that for practical purposes stimulation be defined as "every alteration in the external vital conditions."

Having achieved the new viewpoint (and indeed the word "new" might be omitted, for most of us have none), the reader follows through equally lucid discussions of the characteristics and effects of stimuli, of the process of excitation, of conductivity, the refractory period, fatigue, interference and states of depression, meeting old facts in unexpected places, watching isolated observations falling into line, and finding new meanings in all that is placed before him.

To suggest that the book be read for pleasure is perhaps apparently to belittle its importance. If so the fault lies in the general notion of the meaning of pleasure. But it is undoubtedly true that the biological scientist has few such opportunities for simultaneously pursuing happiness and acquiring merit.

The reviewer had almost forgotten to refer to the excellence of the translation, for which the author makes gracious acknowledgment. The reader will find it very easy to forget that the book was not originally written in English.

C. C. S.

#### REGENERATION OF ANTENNÆ

SOME interesting results have been achieved by experiments, made and reported by H. O. Schmit-Jensen,<sup>1</sup> on the regeneration of severed antennæ of Phasmidæ. A rather large number of small and half-grown larvæ of *Dixippus morosus* had been insufficiently supplied with fresh vegetable food and thus cannibalism appeared among them, and a number were found with one or both antennæ or some of their limbs missing. A single specimen attracted attention because of one of its antennæ having regenerated like a little foot. After the following molt this organ had increased in size and became still more foot-like in form.

This case of spontaneous homœosis caused the author to cut the antennæ from fifty newly hatched and sixty half-grown *Dixippus* larvæ, all the larvæ being from unfertilized eggs. The antennæ were severed between the first and second segments or between the second and third, sometimes the left antennæ being cut and sometimes the right and in some cases both were amputated. When both were cut the specimens died. In some cases where the single antenna was severed there was no regeneration, only a knot developing. But often there was produced, not a small antenna, as one might expect, but a tarsus consisting of from one to the normal five segments complete with terminal claws with the ordinary arolium between them. In four cases a tibia was also developed. In young larvæ there seemed to be a distinct increasing development of the foot-like characters of the regenerated organ with each molt. After the first molt succeeding amputation there appeared only a short knob. The next molt produced a segment with evident claws and the third molt brings the organ into more perfect tarsal formation. Some of the more perfect tarsus-like regenerations are, as shown by figures reproduced from photographs, almost indistinguishable from an actual foot, some, as stated above, even having the tibiæ present. In the older larvæ the place of severance

<sup>1</sup> *Meddel. fra Dansk naturh. Foren.*, Vol. 65, pp. 113-134, Figs. 1-7 (1913).



appears to have some effect, as when the antennæ were cut between the first and second segments nothing but a knot developed but when the cut was made between the second and third segments a foot was regenerated.

A. N. CAUDELL

### SPECIAL ARTICLES

#### A SECOND CASE OF METAMORPHOSIS WITHOUT PARASITISM IN THE UNIONIDÆ<sup>1</sup>

THE discovery<sup>2</sup> three years ago that the species *Strophitus edentulus* (Say) passes through its metamorphosis in the entire absence of parasitism placed that species in a unique position among fresh-water mussels. Since Leydig in 1866 solved the mystery as to the post-embryonic development of the Unionidæ in the discovery that the glochidia are parasitic on fishes, the announcement by Lefevre and Curtis seems to have been the first reported exception.

Lefevre and Curtis<sup>3</sup> in their investigations into methods of propagation of fresh-water mussels found that certain species of fish are more susceptible than others to infection by glochidia. In their operations a number of species of mussel were employed, but the commercially important species were chiefly confined to members of the subfamily Lampsilinæ Ortmann.<sup>4</sup> The fishes found adaptable to infection were the common game fish of the family Centrarchidæ. The fishes which did not take artificial infection were considered by them examples of specific immunity to infection by glochidia.

Following the work of Lefevre and Curtis considerable effort was made to carry through artificial infections with mussels of the genus *Quadrula* (Rafinesque, 1820) Agassiz, a group economically important because of their heavy shells. These attempts, employing the

method of artificially infecting the common and readily obtainable game fish, met with little success. In 1912 I undertook the investigation of this problem. The previous negative results seemed to indicate that suitable fishes were not being used. It seemed probable that the parasitic glochidia, like other parasites, might be considerably restricted as to the species of host to which they were adapted. Working upon this theory I examined considerable numbers of fishes taken at large, with a view to finding those species that were carrying in nature the glochidia of *Quadrula* mussels. These studies supplemented by experimentation in artificial infection confirmed the chief postulate of the theory, namely, that there does exist a decided restriction as to species of hosts for the glochidia of some mussels. In the case of the warty-back mussel, *Quadrula pustulosa* (Lea), for example, I found infection restricted almost exclusively to the Channel catfish, *Ictalurus punctatus* (Rafinesque).<sup>5</sup> The investigation of these natural infections which has been taken up quite extensively by Mr. T. Surber<sup>6</sup> in the mussel investigations by the U. S. Bureau of Fisheries, revealed other points of interest. Among these was noteworthy the entire absence of evidence of infection by some common species. Such observations for a given species of mussel obviously indicate something unusual in the life history. One of the mussels for which I found no natural infection and for which none have been reported was *Anodonta imbecillis* (Say).

During the first part of last November I succeeded in securing several specimens of this mussel. These were all gravid, as is usually to be expected, since this species is hermaphroditic. Upon examining the contents of the marsupium of one individual I found that what at first glance I had supposed were mature glochidia were instead juvenile mussels with organs developed to the stage usually seen at the end of parasitism when the young

<sup>1</sup> Printed by permission of the Commissioner of Fisheries.

<sup>2</sup> Lefevre and Curtis, *SCIENCE*, Vol. 33, pp. 863-865, 1911.

<sup>3</sup> Bulletin of the Bureau of Fisheries, Vol. XXX., 1910 (issued 1912).

<sup>4</sup> *Annals of the Carnegie Museum*, Vol. VIII., No. 2, 1912.

<sup>5</sup> Howard, A. D., *Transactions American Fisheries Society*, 1912, pp. 65-70.

<sup>6</sup> "Notes on the Natural Hosts of Freshwater Mussels," *Bull. Bureau of Fisheries*, Vol. 22, 1912 (issued June 28, 1913).

mussel escapes from its host. These young mussels lie crowded in the marsupial gill of the parent without apparently any matrix or conglomerate structure whatever. The outer gills as in other Anodontas are marsupial and these become well distended throughout their whole length when gravid.

In regard to the breeding of this species Ortmann<sup>7</sup> says it is gravid from September to May. My observations, which are rather limited on this point, I give below:

Place	Date	No. of Individuals	Stage of Gravidity
Fairport, Iowa	Feb. 2	1	Early embryo
" "	May 13	1	Glochidia
" "	May 27	1	Glochidia
" "	July 16	1	Glochidia
Moline, Ill.	Sept. 24	1	Not gravid
" "	Nov. 7	2	Early embryos
" "	Nov. 7	1	Late embryos and glochidia
" "	Nov. 7	6	Juveniles

In addition to these I have found numbers of free juveniles not sexually mature ranging in length from 5 to 30 millimeters. These stages are remarkable for the thinness of their shells and the flatness of the mussel as a whole. The term "floater," of the mussel-fishermen, for this type of mussel is well applied in its use for this immature stage.

The presence of juveniles in the marsupia during November in a majority of the specimens examined seems to indicate that metamorphosis is probably completed in the fall. The time of discharge of the young mussels is yet to be determined but the appearance of glochidia again in early spring would seem to indicate that the juveniles escape in the fall or early winter.

Among the six lots of marsupial juveniles that I collected the degree of development varied slightly as to amount of shell growth, otherwise there seemed to be little difference. This growth consists of a narrow rim only, around the edge of the glochidial shell. The hooks of the glochidium are still much in evidence but are much weaker than in parasitic forms. A noticeable feature is the large pro-

portion of gaping shells as compared with a similar lot of glochidia. It would seem that with the loss of the powerful single adductor muscle the action of closing is less vigorous. Between the gaping valves can be seen the ciliated foot, two adductor muscles, the mantle, on each side the gill papillæ, etc., indicating a development equal to that of other young Naiades at the end of parasitism.

I have tested the reaction of the glochidia in the presence of fish and obtained strong evidence that they do not respond as other known parasitic forms. Mature glochidia taken in March were employed; in an exposure to fish for an hour they failed to give the usual infection. A few glochidia lodged in the mouths of the fish but no encystment could be detected. The fish showed no response. Following this test the fish were exposed for ten minutes to the glochidia of *Symphynota complanata* (Barnes). These rapidly became attached and the fish showed considerable uneasiness in marked contrast to their indifference in the presence of the other glochidia.

From these observations I think we are warranted in concluding that this mussel passes through its metamorphosis in the entire absence of parasitism. The period immediately succeeding this metamorphosis has not been followed but there seems to be little reason for suspecting any parasitism here.

In *Strophitus edentulus* the mussel for which Lefevre and Curtis found a non-parasitic metamorphosis the arrangement of the glochidia in the gills is very unusual as has been described by them and other authors. The glochidia at first and later the juveniles are imbedded in cords of a gelatinous semi-transparent substance which lie like crayons in a box packed in the water tubes of the marsupium. Under natural conditions these are shed into the water from time to time. Sterki<sup>8</sup> called these cords placenta and Lefevre and Curtis<sup>9</sup> have concluded that they

<sup>8</sup> "Some Observations on the Genital Organs of Unionidæ," *Nautilus*, Vol. 12, pp. 18-21 and 28-32.

<sup>9</sup> *Op. cit.*

<sup>7</sup> *Op. cit.*



have a nutritive function. The absence of a placenta or any matrix about the glochidia of *Anodonta imbecillis* is of interest since the non-existence of parasitism in this case is apparently under quite different conditions from those governing in *Strophitus*. I have mentioned above the extreme lightness of the juvenile shells in *Anodonta imbecillis* up to a considerable size. In the resulting buoyancy we have undoubtedly a device for distribution of the young and thus a compensatory provision for the loss of the usual means of distribution by fishes.

At the U. S. Fisheries Station, Fairport, Iowa, there are several ponds used for retaining fish seined from the Mississippi River. In these ponds have been found a great many young mussels of species known to be parasitic on fish and evidently introduced into the ponds during the parasitic stage. A concrete reservoir was at first used to supply the water to the ponds. Upon examining the bottom of this reservoir in 1912 the presence of mussels (Unionidæ) was discovered. This at first seemed surprising as no fish had been put in the reservoir, but it was noteworthy that these mussels were all of one species, *Anodonta imbecillis*. The explanation given for their presence was that owing to the lightness of their shells in the juvenile stage they had been pumped through the intake pipe from the river. This explanation made without the knowledge of the non-parasitic metamorphosis was undoubtedly the correct one and I give the incident only as an illustration of the possibilities of their distribution in water currents. It is my opinion that the so-called "placenta" of *Strophitus edentulus* has a similar distributing function; the cords being buoyant may be readily carried by flowing water. In this case, however, the mechanism is quite different and thus we have in the two species different devices for accomplishing the same purpose.

The question arises as to the nutrition of these non-parasitic glochidia during the period of metamorphosis. Both of these species undoubtedly have come from parasitic ancestors

which received at this stage nutriment from their hosts so that one would look for some provision for nutrition here.

I have not as yet observed any such provision in *Anodonta imbecillis* and I do not know that this has been demonstrated for *Strophitus*. In the latter case to prove a nutritive function for the cords it would seem necessary to demonstrate an absorption of the substance of the cords by the young mussels. As the cords swell considerably upon leaving the gills such a determination is difficult.

The discovery of so fundamental a change of habit, apparently derived independently by two lines, should give opportunity for many interesting comparisons; for *Anodonta imbecillis* already possessing the distinction of being an hermaphroditic species it adds another eccentricity to its reputation.<sup>10</sup>

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#### LABORATORY NOTES

##### I. EMBEDDING TRAYS

IN the laboratories of this country and Europe a variety of receptacles are used to hold the melted paraffin in embedding. Doubtless all of them have certain advantages and it is certain that most of them have annoying disadvantages. Paper trays are not stiff enough for large cakes and are very likely to stick. L-shaped bars of metal that can be adjusted to a variety of sizes are placed on glass plates. They are very likely to leak if the paraffine must be kept liquid any length of

<sup>10</sup> Since the above was written I have been able to secure infections and encystment on fishes with *Anodonta imbecillis* as well as *Strophitus edentulus*. In the latter complete metamorphosis was observed. Thus for *edentulus* we have indicated facultative parasitism while in the other we have a persistence of the parasitic reaction at least when artificially brought in contact with a host. Metamorphosis on fishes was not secured in *A. imbecillis*. Abundant additional evidence is at hand that development in this (*imbecillis*) species normally proceeds without parasitism.

time to make the proper orientation of the objects to be embedded. The paraffine frequently sticks to the glass unless considerable care is taken to keep it absolutely clean and to smear it carefully with glycerine or albumen fixative. Metal and porcelain dishes have the same likelihood of sticking, but are otherwise excellent.

The author owes to Dr. Hally D. M. Jollivette the suggestion that has led to our present laboratory practise. Her original suggestion was to make handmade trays of plaster of paris. This was tried with excellent results except that the trays break very easily. This led the author to seek for a substitute that would retain the advantages of plaster of paris but would be less fragile. After a number of experiments it was found that dishes made of the same sort of *unglazed* earthenware as flower pots answer all the requirements.

The advantage of these earthenware dishes are that they can be dipped in water until thoroughly saturated so as to be entirely impervious to paraffine. Danger of sticking is thus entirely obviated unless one carelessly overheats them. If the water is driven out by heating, the paraffine, of course, penetrates the porous clay and renders the dish useless until it has been dissolved and completely removed. I have found that the best results can be achieved in handling large quantities of materials by keeping the dishes in a vessel of water a few degrees warmer than the melted paraffine. When one is wanted, remove it from the warm water to a position on the warming stand that will prevent its cooling off too rapidly. The objects can then be oriented at one's leisure. To cool the paraffine set the tray of melted paraffine in a dish of cold water until hard enough to immerse. As soon as the cake has hardened it will float out without any difficulty.

## II. TURPENTINE AS A LABORATORY REAGENT

THE waste of expensive laboratory reagents by elementary students, who do not know their value, is oftentimes a considerable annoyance to the instructor in histology or other subjects where students must be allowed more or less

ready access to the stock room. Aside from waste the economy of reagents is a matter of no inconsiderable importance to the directors of most laboratories. Such considerations as the above have influenced us in trying various experiments in substitution.

Commercial turpentine, such as is sold by the hardware store to painters, has been found a valuable substitute for other much more costly reagents. In fact, for many purposes it is superior to the much more expensive article purchased from the chemical-supply house.

Most laboratories use turpentine for dissolving the paraffine after the ribbons have been fixed to the slide. While this usage is comparatively widespread the practise of using it in place of xylol, oil of bergamot, etc., for clearing preparatory to embedding in paraffine appears to be less frequent. After much experience with these various reagents the author is convinced that it is not only vastly cheaper, but that it is on the average quite the equal of any of the others. It penetrates freely and dissolves proportionally as much or more paraffine. The specimens of plant materials clear readily, infiltrate quickly, and cut as well as if embedded through other reagents.

I have found it actually superior to other reagents in clearing sections. It clears readily from 95 per cent. alcohol and so avoids the use of absolute alcohol. Both time and expense are saved in this way. Slides and sections should, however, be rinsed in xylol before being mounted in balsam. Some stains are soluble in turpentine and so slides must not be left overlong in it unless they are overstained. It is valuable in reducing overstaining from aniline blue and bismark brown.

The ease and convenience of handling wood sections and celloidin sections in which it is desirable to retain the celloidin is an enormous convenience. Sections can be transferred to turpentine from 95 per cent. alcohol. In the former one step in the process is saved and in the latter the danger of dissolving or softening the celloidin is avoided.

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